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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

SIR WILLIAM THOMSON, KNT. LL.D. F.R.S. &c.

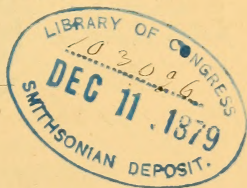
AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. l. Not.*

VOL. VIII.—FIFTH SERIES.

JULY—DECEMBER 1879.



LONDON:

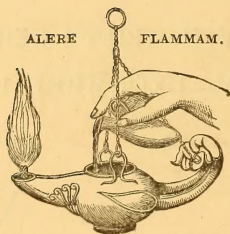
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“Meditationis est perscrutari occulta; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.



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[FIFTH SERIES.]

JULY 1879.

- I. *On the Passage of the Galvanic Current through Iron.*
By FELIX AUERBACH, Ph.D., of Breslau*.

[Plate I.]

THE characteristic enormous value of the specific magnetism of iron is, as investigations accomplished in the last decades have shown, not without influence upon the galvanic peculiarities of that metal; for if a current be conducted through an iron wire, phenomena make their appearance which do not occur with other metals. Some of these phenomena shall in the following be submitted to a consideration based on new experiments, and judged from a unitary point of view which, I am of opinion, has hitherto been missing in the literature of this subject.

I commence with a brief comparison of the known facts, so far as I shall have to refer to them.

§ 2. (1) The statements respecting *galvanic conductivity* vary within comparatively wide limits, even when those which can be impugned are excluded. Taking, namely, that of silver as equal to 100, the corresponding number for iron was found by

E. Becquerel	(1846),	$\lambda = 12.35$
Bénoit †	(1873),	12.7
Lenz	(1838),	13.1
Pouillet	(1846),	14.1
Matthiessen	(1858),	14.44
Buff	(1857),	14.77
Arndtsen	(1858),	14.83
Frick and Müller	(1848),	15.9

* Translated from the original Essay (Leipzig, 1878), communicated by the Author.

† *Comptes Rendus*, t. lxxvi. p. 342; *Phil. Mag.* [IV.] vol. xlv. p. 314 (1873), and xlix. p. 78 (1875).

Phil. Mag. S. 5. Vol. 8. No. 46. July 1879.

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(2) *The conductivity diminishes, or the resistance increases, as the temperature rises.* If we put

$$\lambda_t = \lambda_0(1 - \alpha t + [\alpha^2 + \beta]t^2)$$

and

$$w_t = w_0(1 + \alpha_1 t - \beta_1 t^2),$$

then is, at all events very nearly,

$$\alpha_1 = \alpha \text{ and } \beta_1 = \beta.$$

Therefore

$$\beta_1 = 0 \text{ in case } \beta = 0.$$

This latter relation was found by E. Becquerel, Arndtsen (approximately), and Mousson. At the same time

Becquerel gives	$\alpha = 0.004726$
Arndtsen . .	0.00413
Mousson . .	0.004207

On the contrary, Matthiessen finds β_1 different from 0; namely, from

$$\lambda = \lambda_0(1 - 0.0051182t + 0.000012915t^2)$$

follows

$$w = w_0(1 + 0.0051182t - 0.000013281t^2).$$

But here also we can, with very close approximation, regard the formula

$$w = w_0(1 + \alpha t)$$

as satisfied. This assumption, namely, leads to the equation

$$\lambda = \lambda_0(1 - \alpha t + \alpha^2 t^2 - \dots)$$

or

$$\lambda = \lambda_0 \left(1 - \alpha t + \frac{\alpha^2 t^2}{1 + \alpha[t]} \right),$$

in which $[t]$ signifies a mean value of t , which can be introduced, in that term of the correction, in place of the true one. It is true that the statements of the observer furnish no certain support for this; but if it be put, in a round number, equal to 200°C. , we get

$$\lambda = \lambda_0 \left(1 - \alpha t + \frac{\alpha^2 t^2}{2} \right),$$

as in fact Matthiessen found (nearly).

(3) The quantity of heat generated by the current in an iron wire is approximately determined by Joule's law. I have not been able to discover any numbers referring to this more accurate than the old ones of Lenz, which, on account of the temperature rising with the intensity of the current, are not

exactly comparable. The mean values of the times required for equal heatings are :—for

Copper	478·9
German silver . .	460·4
Platinum	451·7
Iron	448·0

(4) When we close a circuit consisting of a battery and a rectilinear iron wire, we observe an extra current in the opposite direction to that of the principal current*. At the opening of the circuit an extra current arises with the same direction as the principal current. These currents were discovered by Villari, and were named shock-currents. G. Wiedemann† at first based an explanation of them on the assumption of a transversal or *circular magnetization* of the iron wire; Herwig‡ and Streintz§ have since engaged in a more detailed investigation of them.

(5) *Longitudinal magnetizing of iron rods or wires has an influence upon their resistance.* I mention this point last because the results hitherto obtained respecting it have proved most anomalous and in part contradictory to one another. Edlund and Mousson found *no alteration*, Thomson|| and Beetz¶ *increase* of the resistance; while even a *diminution* was inferred from older observations, and has recently been confirmed. Beetz's results have lately been corroborated by Chwolson** in a brief communication.

§ 3. In the experiments and considerations which I have carried out upon the facts here brought together, I started directly from the last two points, partly because here the material in hand appeared to me least sufficient for the full understanding of the phenomena, partly because I considered that I could not concur in the view (in which Beetz and Herwig agree) that the cases treated by them (4 and 5) must be kept perfectly distinct. Rather have I, with G. Wiedemann††, received from their results the conviction that the two phenomena may very well be connected with one another, *and hence are to be contemplated from a common point of view.*

* The current made use of for the measurement, passing through the iron, I shall constantly in the following designate as the principal current.

† *Galvanismus*, 2nd ed. ii. 2, § 743 (1873).

‡ *Pogg. Ann.* vol. cliii. p. 115 (1874).

§ *Wien. Ber.* vol. lxxvi. (1877).

|| *Phil. Trans.* 1856, p. 737.

¶ *Pogg. Ann.* vol. cxxviii. p. 202 (1866).

** *Carl's Rep.* vol. xiii. p. 232 (1877).

†† *Galvanismus*, 2nd ed. ii. 1, p. 593.

I commence with the examination of the influence of magnetization upon the resistance.

For measuring the resistance I used a Wheatstone bridge. A universal resistance-box by Siemens furnished the resistances w_1 , w_2 , w_3 of the three parallel branches. In the experiments the ratio $w_1 : w_2$ was almost constantly as 1000 : 10, in some instances as 100 : 10; consequently the resistance w_3 in the first case represents 100-fold the resistance w_4 to be measured. Since 0.01 of a Siemens unit can be measured with certainty, the accuracy of the determination extends to the fourth decimal place; it only became uncertain in case w_4 amounted to more than 10 units.

I at first used as the measuring-instrument a Wiedemann galvanometer with the thinnest of the wire coils that are usually joined to it, afterwards one constructed by Sauerwald according to Magnus' plan. By means of mirror-reading I could estimate with certainty 0.1 of a scale-division (= 0.1 millim.). The last-mentioned instrument is highly suitable for investigations in which rapidity of observation is important, on account of the quick suppression of the vibrations.

The first and most difficult problem was, *how to exclude the influence of variations of temperature*; for when we consider that according to the statements of Thomson, Beetz, and Chwolson the upper limit of the alterations of resistance by magnetizing is given by the ratio 1 : 1000, and that an alteration of resistance corresponding to this limit value is produced in iron by a change of temperature of 0.2 C., it is evident what careful attention ought to be directed to this point. The effects of temperature can either be set aside by a suitable arrangement of the experiment before the commencement of the observations, or eliminated by calculation after their conclusion. In regard to that portion of those influences which is produced by the principal current itself, after some preliminary experiments I decided for the latter. Of course, in the equation of the Wheatstone bridge, I could easily have made the ratio $w_4 : w_3$ independent of the temperature by having the resistance w_3 for the most part of iron, and taking only the small part requisite for the actual regulation out of the resistance-box. But since the ratio $w_2 : w_1$ was = 1 : 100, iron wires of considerable length would have been necessary, which for other reasons I was obliged to avoid. Moreover the heating produced by from 1 to 3 Daniell elements, such as I used for the principal current, during the mostly very short time that the current was closed, was extremely little; and it is well known that with small values of the foreign influences (of

whatever kind they may be) elimination furnishes very reliable results. Thereby the scheme of all the experiments which I made assumed the form $a_1 - b - a_2$, where a_1 signifies the experiment before the magnetization, b the same after magnetizing, and a_2 after demagnetizing. The results b and $\frac{a_1 + a_2}{2}$

are then fairly comparable if the difference $a_1 - a_2$ is small.

A second portion of the temperature-influences, that occasioned by the surroundings, can also be reduced to a minimum, and that minimum eliminated. The latter can here be done with peculiar facility, since the periods of the extreme variations of temperature stand in no connexion whatever with those of the alterations of the resistance conditioned by the experiments.

There still remains for discussion the *third* and most important part of the influences of temperature, namely those conditioned by the *magnetizing-arrangement*. This in the beginning consisted of a spiral of thick wire, spun over and waxed, and wound upon a glass tube, through which the magnetizing current flowed. The iron wire was pushed into the glass tube, and the entire apparatus set up at a distance of several metres from the galvanometer; so that neither the magnetism nor even the magnetizing current acted directly upon the galvanometer. It nevertheless appeared that the heating action of the magnetizing current was conveyed by radiation from the spiral to the iron wire and occasioned variations in its resistance, which, partly by their considerable amount, and partly because the periods of both variations are identical, concealed all the alterations which were to be measured*. Hence it was important to construct an adiathermanous magnetizing-apparatus. In this I succeeded by making use of the procedure often employed by Joule in his thermal investigations—namely, placing in layers one over another a number of series, each consisting of an adiathermanous, a badly conducting material, and a material of great heat-capacity. The copper spiral was accordingly wound upon a wide glass tube, this drawn over a thick-walled caoutchouc hose; inside this a narrower glass tube was inserted, and in the latter the iron wire, mostly wrapped round with paper. To carry out the principle completely I should have had to put between the narrower glass tube and the iron wire a layer of substance of great heat-capacity—for example, a stationary stream of alcohol or melt-

* These variations of resistance furnish a very serviceable method of following the temperature in a galvanic battery. I intend to return to this subject in another place.

ing ice. Recently, however, experiments have been made on dielectric, diamagnetic, and electrolytic influences of such surrounding liquids, which appeared to me in the present case not to be neglected; and an apparatus so constructed was sufficient for two of the magnetizing forces which I used—a Daniell and a Bunsen battery of five cells at the most. With the third (the current of a Gramme machine driven by a steam-engine), it is true that in most cases sensible thermal effects still remained; they were, however, small enough to admit of elimination. The employment of such powerful magnetizing forces may perhaps in general appear to be superfluous, since it is usually assumed that iron would be magnetized to so-called saturation by much feebler forces. But I should doubt whether in such cases it is actually attained; it is probably “nearly” reached*. Perhaps the molecular magnets deviate, in the mean, only a fraction of a degree from the axis of magnetization; but just the influence of the still possible twist may, in reference to such questions as these here to be examined, be material.

With the arrangement adopted there was no fear of thermocurrents; but it was necessary to reduce as much as possible the intensity of the above-mentioned extra currents. The necessity of this will not, perhaps, be at once obvious. In the present investigation, however, lasting influences are in question; and from these those momentary phenomena must be easily distinguishable. But, in the first place, it is for many reasons desirable to be able to determine also these lasting influences immediately after the closing of the current, in order that it may soon be opened again; and, secondly, those extra currents are any thing but momentary phenomena. I have much rather found that here the phenomena known under the name of after-effect play a great part. This is the more disagreeable, as the still remaining temperature-influences continue to operate according to the time occupied by the radiation and conduction. I have, on this account, operated throughout with iron wires, in which Villari’s currents appear far less intense than in rods.

§ 4. It is *à priori* to be expected that the nature of the wire (whether it consists of steel or iron, whether it is hard or soft, &c.) will have some influence on the phenomena exhibited by it, that a wire which has not yet been galvanically operated on behaves differently from one through which, during a longer or shorter time, currents have passed, or which has already repeatedly undergone longitudinal or transverse magnetization. But to this must be added an essential moment, con-

* Thus M. Beetz cautiously expresses himself.

sisting in the ratio of the intensities of the magnetizing and the principal current, and to which I shall further on attach my theoretic considerations. From these causes the numbers of the following Tables show great diversity.

Of the series of experiments with weak magnetizing currents, some are already serviceable before the improvement of the magnetizing-apparatus. I denote by:—H the electromotive force generating the principal current, M that producing the magnetizing current; D the electromotive force of a Daniell, B that of a Bunsen element; n the number of turns of the magnetizing spiral; l the approximate length, d the thickness, of the iron or steel wire; w_0 the resistance of the same, measured in Siemens units, for $M=0$, w the same for $M>0$; w_k the resistance of the copper conducting-wires to the resistance-box; δ the quotient $\frac{w-w_0}{w}$; t the time occupied by the experiment*.

Passing over the time-details of the phenomena, I first give only the numbers hereto belonging.

1. $H=M=1D$; $n=106$; (annealed iron wire f_1)
 d = about 0.5 millim.

$w_0 + w_k$	$w + w_k$	$w_0 + w_k$	$(w_k = 0.5209)$
1.0300	1.0296	1.0304	$\delta = -0.0012.$

2. H, M, n , as before. $f_1.$

$w_0 + w_k$	$w + w_k$	$w_0 + w_k$	$(w_k = 0.5211)$
1.0304	1.0294	1.0302	$\delta = -0.0018.$

3. H, M, n as before. (Annealed iron wire f_2) d about 0.5 millim.

$w_0 + w_k$	$w + w_k$	$w_0 + w_k$	$(w_k = 0.5210)$
1.7478	1.7459	1.7476	$\delta = -0.0015.$

4. H and M as before; $n=166$. Wire $f_1.$

$w_0 + w_k$	$w + w_k$	$w_0 + w_k$	$(w_k = 0.5201)$
1.0227	1.0216	1.0229	$\delta = -0.0024.$

Mean of 1-3, $\delta = -0.0015$, while in 4 $\delta = -0.0024$.

The magnetizing force is proportional to n : according to the above, therefore, δ is proportional to the magnetizing force. According to its sign, δ is negative. The length of the wire (experiment 3) appears to be without influence.

* Unfortunately, in most of the older experiments some of these data are wanting, it not having been anticipated that they would be required.

5. $H=3D$; $M=1D$. Wire f_2 . $n=106$. The magnetizing current remained closed for an hour.

$$w_0 + w_k = 1.7429 \quad w + w_k = 1.7326 \quad w_0 + w_k = 1.7386 \\ (w_k = 0.5207); \text{ therefore } \delta = -0.0068.$$

Here, therefore, first a greater transient, secondly a permanent diminution of the resistance took place, although there was no demonstrable permanent magnetization. The temporary diminution becomes still greater, if in its calculation we neglect the permanent—that is, in calculating the difference $w - w_0$, keep in view only the initial w_0 . Then we have

$$\delta_1 = -0.0084.$$

6. $H=5D$; $M=1D$; $n=106$. Wire f_2 .

$$w_0 + w_k = 1.7366 \quad w + w_k = 1.7139 \quad w_0 + w_k = 1.7299 \\ (w_k = 0.5210); \text{ therefore } \delta = -0.0159.$$

Neglecting the permanent alteration, we get

$$\delta_1 = -0.0186.$$

Here the temporary alteration of the resistance is still greater, its value reaching nearly 2 per cent. But the permanent alteration is also greater than in the preceding case.

Some series of experiments on unannealed iron wires exhibit the same phenomena, only more feebly. On the contrary, with a thin unannealed steel wire the resistance diminished nearly 3 per cent. when $H=3B$, $M=1D$ were chosen.

Most of the experiments made before the improvement of the electromagnet, however, show an increase of the resistance with magnetization, and, indeed, a greater temporary and a smaller residual increase. As, however, in them it is hardly possible to separate the influence of the increase of temperature, I have, with the exception of a few apparently reliable ones, made no use of those experiments.

7. I will next mention two experiments with thin unannealed iron wires, in which no alteration amounting to 0.0002 per unit of the resistance occurred. In both was $H=2D$, $n=92$; further, in one of them $M=3D$, in the other $M=2B$.

8. Annealed iron wire f_3 (d about $=0.6$). $H=1D$; $M=3D$; $n=92$. The resistance of the conduction-wires is already deducted.

$$w_0 = 0.5213 \quad w = 0.5234 \quad w_0 = 0.5214 \quad \delta = +0.0038.$$

9. Experiment 8 several times repeated.

$$w_0 = 0.5214 \quad w = 0.5232 \quad w_0 = 0.5214 \quad \delta = +0.0035.$$

The influence of the repetition many times of the same experiment rarely showed itself so insignificant as in this case.

10. Instead of the resistance-box, a rectilineal rheostat was employed. The numbers for w_0 and w are expressed in terms of an arbitrary unit. $H = 1D$; $M = 2D$; $n = 92$. Mean values:—

$$\left. \begin{array}{l} [w_0] = 6019.8 \\ [w] = 6023.2 \end{array} \right\} [w - w_0] = +3.4,$$

$$\delta = \frac{w}{w_0} \cdot \frac{100000 - w_0}{100000 - w} - 1 = +0.00135.$$

The total resistance of the rheostat is put equal to 100000; the above calculation was necessary, because here equilibrium was restored by shifting a binding-screw.

11. In experiment 10 the principal current passed in the iron wire from the south to the north pole. The principal current was now sent through in the opposite direction. The mean values found were:—

$$[w_0] = 6021; [w] = 6024; [w - w_0] = +3; \delta = +0.00122.$$

(According to the same reckoning as above.) Here, however, it must be noted, first, that the iron wire with which these two series of experiments were made had already been repeatedly passed through by the current, and, secondly, that, in the calculation of the mean values in 11, the first numbers, deviating, were left out of the account. The complete series, namely, are as follows:—

$$\begin{array}{l} 10. \left. \begin{array}{l} [w_0] = 6019 \\ [w] = 6024 \\ [w_0] = 6021 \\ [w] = 6023 \\ [w_0] = 6019 \\ [w] = 6022 \\ [w_0] = 6020 \\ [w] = 6024 \end{array} \right\} \text{therefore } w \text{ constantly } > w_0. \\ \\ 11. \left. \begin{array}{l} [w_0] = 6022 \\ [w] = 6020 \\ [w_0] = 6022 \\ [w] = 6025 \\ [w_0] = 6022 \\ [w] = 6024 \\ [w_0] = 6020 \\ [w] = 6023 \\ [w_0] = 6020 \end{array} \right\} \begin{array}{l} w < w_0 \\ \\ w > w_0 \end{array} \end{array}$$

The reversal of the direction of the current in the iron has

therefore for its consequence a momentary but no permanent change of sign of δ . That the repeated opening and closing of the magnetizing current exercised no influence depended probably on stationary relations having already entered, in consequence of the wire having been long in use. In order to test this supposition I gave to a yet unused iron wire of like constitution the same dimensions, and obtained:—

$$\begin{array}{rcccccc} 12. & [w_0] = & 6018 & 6020 & 6017 & 6015 & 6013 \\ & [w] = & 6011 & 6011 & 6008 & 6006 & \end{array}$$

$$\text{Means:—} [w_0] = 6016.6; \quad [w] = 6009.0; \quad [w - w_0] = -7.6; \\ \delta = -0.0032.$$

Here, therefore, δ is negative. In fact, after I had closed and opened the magnetizing current about 100 times, the absolute value of δ diminished, and on the next day I obtained, as in 10 and 11, a small positive value of δ . But I have only observed such change of sign of δ when, for $n=92$, the ratio $H : M = 1 : 2$ or did not much differ from this. Only when very intense currents had passed through the wire for a longer time, or when I had repeatedly reversed the direction of the magnetization, did the results become in other cases also and completely irregular.

Shortly before the construction of the adiathermanous apparatus, I made some more experiments, in which, it is true, I verified an influence of the heating; but I believed I should be able to eliminate it by comparison with experiments on copper wires. The specific heats, namely, of iron and copper are to one another about as 7 : 6; and, according to Benoit, their temperature-coefficients α are approximately in the same ratio for the galvanic resistance. If, then, we employ the same value of M and the same magnetizing-apparatus, and give to the surfaces of the iron and the copper wire equal magnitude, by which also their volume becomes the same, we get in both nearly equal augmentations of resistance by radiation from the magnetizing-apparatus. Hence I proceeded in the way indicated, and obtained (the indices f and c refer to the soft-iron and the copper wire respectively):—

$$13. \text{ Resistance-box. } H=1D; M=2B; n=73.$$

$$\delta_c = 0.0023 \quad \delta_f = 0.0042$$

$$\delta_f - \delta_c = \delta = +0.0019.$$

$$14. \text{ The same repeated.}$$

$$\delta_c = 0.0026 \quad \delta_f = 0.0039 \quad \delta = +0.0013.$$

$$15. H=1D; M=3B; n=73, \text{ thicker wires.}$$

$$\delta_c = 0.0017 \quad \delta_f = 0.0044 \quad \delta = +0.0027.$$

Here δ has increased, in comparison with experiment 13, exactly in the ratio of the magnetizing force.

The following series of experiments were all made with the adiathermanous apparatus.

With unannealed iron wires the residual effect of the extra currents after the closing of the magnetizing and the principal current is very protracted; so that here we can often only with difficulty separate the transient from the permanent phenomena. Hence I designate by w_1 the resistance in the first period (often amounting to some minutes) after the closing of both currents, by w_2 the resistance that has become constant.

16. $H=1D$; $M=3B$; $n=150$. Iron wire f_4 (d about 0.60). Mean values:—

$$w_k + \begin{cases} w_0=0.79046 \\ w_1=0.79107 \\ w_2=0.79052 \\ w_0=0.79044 \end{cases} \begin{cases} w_k=0.0698 \\ w_1-w_0=0.00061 \\ w_2-w_0=0.00007 \end{cases}$$

$$\frac{w_1-w_0}{w_0} = \delta_1 = +0.00084$$

$$\frac{w_2-w_0}{w_0} = \delta_2 = +0.00010.$$

17. The same repeated.

$$w_k + \begin{cases} w_0=0.78998 \\ w_1=0.79050 \\ w_2=0.79010 \\ w_0=0.78998 \end{cases} \begin{cases} w_1-w_0=0.00052 \\ w_2-w_0=0.00012 \end{cases}$$

$$\delta_1=0.00072$$

$$\delta_2=0.00016.$$

Therefore, on the repetition of the experiment, δ_1 has become less, δ_2 has increased. With this agrees the fact that after being used many times wires mostly give no perceptible difference between δ_1 and δ_2 .

18. $H=1D$; $M=3B$. Strong iron wire f_5 (d about 1.3).

$$w_k + \begin{cases} w_0=0.16617 \\ w_1=0.16640 \\ w_2=0.16630 \\ w_0=0.16613 \end{cases} \begin{cases} w_1-w_0=0.00024 \\ w_2-w_0=0.00016 \end{cases}$$

$$\delta_1=0.00260$$

$$\delta_2=0.00156.$$

Here not only are δ_1 and δ_2 absolutely greater than in the two preceding series, but δ_2 is much greater in comparison with δ_1 . There only one fifth part of the initial increase of resistance was permanent; here, more than half.

19. Strong iron wire, annealed, f_6 . $H=1D$; $M=4B$; $n=212$. Mean values.

$$w_0=0.5029 \quad w=0.5171 \quad w_0=0.5078$$

$$\delta=0.0240.$$

Permanent increase $\Delta=0.0097$. If we neglect this, we get for the transient increase

$$\delta=0.0281.$$

These values are extraordinarily high. The permanent increase of the resistance can also be inferred from the fact that we have here to do with abnormal proportions. Of course this conclusion must, to a certain extent, be extended to the following series of experiments, based upon another wire.

20. H and M as in 19. A thin iron wire, annealed, f_7 , several times operated on galvanically; $d=0.19$.

$$w_0=6.3549 \quad w=6.4532 \quad w_0=6.3644$$

$$\delta=+0.0147.$$

Further, with the same signification as above, we get

$$\Delta=0.0015 \text{ and } \delta_1=0.0156.$$

The disproportionately low value of Δ , compared with that found in 19, can be accounted for from the circumstance that the wire f_7 had already, in consequence of having been frequently passed through by the current, considerably neared its stationary condition.

21. $H=2D$; $M=3B$. A thin hard iron wire f_8 ; $d=0.15$.

$$w_0 + w_k = 11.6142.$$

No alteration of resistance, not even 0.0001, although here, in spite of the very slight thickness, the extra currents corresponded to a considerable increase of resistance for the moment of the closing of the current.

22. $H=2D$; $M=1D$; $n=92$. Hard iron wire f_9 ; $l=1670$, $d=0.20$.

$$w_0=7.3961 \quad w=7.3903 \quad w_0=7.3973$$

$$\delta=-0.0009.$$

The same wire, however, showed after longer working, after δ had constantly approached nearer and nearer to zero, at last small positive values of δ . In the meantime it was submitted to experiment 23.

23. $H=1D$; $M=2D$. Wire f_9 .

$$w_0=7.3991 \quad w=7.3997 \quad w_0=7.3993$$

$$\delta=-0.0002.$$

24. Repetition of 22. As mentioned, it gave an augmentation, though hardly sensible, of w_0 . For a control I produced, out of the same coil of wire from which f_9 had been taken, a new wire of the same dimensions, nearly, and found for $H=2D$, $M=1D$ (therefore as in 22):—

$$w_0=7.3608 \quad w=7.3559 \quad w_0=7.3602$$

$$\delta = -0.0006,$$

therefore a negative value of δ , as in 22.

25. In experiment-series 6 and 21 the conclusion was reached that the resistance of hard iron wires was not changed by magnetizing them. I sought to realize the same case with a tempered thick steel wire F_1 ($l=1450$, $d=1.31$). I succeeded, when H was $=2D$ and $n=92$, with almost equal approximation when M was $=2D$ and $M=3D$. This point can be fixed still more exactly with annealed iron wires. On the contrary, in hard steel wires there was always an alteration of the resistance; and, indeed, with H constant, the more insignificant the magnetizing force the smaller in general was the alteration. Of the experiments with slight magnetizings I will only mention that they mostly yielded lower values of δ than those with iron wires; only with the lowest values of the ratio $Mn : H$ does the opposite take place. On the other hand, I will give some more series of experiments with magnetizing forces to which, in iron and tempered steel, positive values of δ would have corresponded.

26. $H=1D$; $M=3B$; $n=110$. Wire F_3 of English steel (music-steel). $l=1280$; $d=0.91$. Mean values (the first experiments being excluded):—

$$w_k + \left\{ \begin{array}{l} w_0=0.40091 \\ w=0.40079 \end{array} \right\} w - w_0 = -0.00012$$

$$(w_k=0.050 \text{ nearly}).$$

$$\delta = -0.00034.$$

If now the magnetizing current was reversed, there followed, as at the commencement of the first series, first an increase of δ by magnetizing, which, however, after the current had passed twice had already given place to a diminution. Generally, this wire proved very favourable, not only for these measurements, but also for those of which I have yet to speak in the course of this investigation. But the absolute values of δ are here much smaller than in other wires; already on the 25th May, with a series (M) of 5B, I had obtained scarcely higher values of δ .

In order to determine whether the cause of this was the

slight dependence of the resistance on the magnetization of the English steel, or whether considerably stronger magnetizing forces were not required here in order to produce, even approximately only, the state of so-called saturation, I made use of a Gramme machine of the newer construction (1875), a short time previously acquired for this laboratory by Prof. O. E. Meyer, and, driven by a steam-engine, capable of operating with considerable effect. Its distance from the galvanometer amounted to about 25 metres; the electromagnet was set up between the two, at the distance of 5 metres from the machine, and 20 from the galvanometer, and connected in both directions by thick copper wires. The spiral never grew so hot that it could not be touched with impunity. The caoutchouc pipe never exhibited any trace of heating, even with the most rapid rotation of the machine (1200 revolutions per minute). Notwithstanding, many of the series of experiments made with soft iron wires, especially with thin ones, proceeded in a manner which I can only account for by influences of temperature. If we seek to eliminate these, we obtain increases of resistance by magnetization rising to 1 per cent. and over. Simpler and more reliable are the results of the experiments with steel wires, the mean values of which are here extracted:—

27. $H=1D$; M = Gramme machine (number of revolutions, $p=700$); $n=110$. Thin steel wire F_5 , $l=2000$, $d=0.21$.

$$w_0=8.4960 \quad w=8.4803 \quad \delta=-0.0019.$$

28. The same repeated (4 series of experiments).

$$w_0=8.6415 \quad w=8.6346 \quad \delta=-0.0008.$$

29. Steel wire F_3 . $H=1D$; M = Gr. machine ($p=800$).

$$w_0=0.3435 \quad w=0.3419 \quad w_0=0.3435 \quad \delta=-0.0047.$$

30. To this experiment was joined immediately a control experiment with a copper wire. (Such control experiments were also previously made from time to time.) $H=1D$; M = Gr. machine ($p=1000-1200$); $l=2220$; $d=0.70$. Mean values:—

$$w_0=0.14138 \quad w=0.14149 \quad w_0=0.14144 \quad \delta=+0.00056.$$

For copper wire, therefore, δ is, first, positive, and, secondly, much smaller in absolute value than in 29, although p is much greater. Besides, all the other experiments with copper wires have yielded considerably lower values still of δ . If these alterations of resistance should prove to be connected with the magnetic behaviour of copper, they will disprove the generality

of the diminution of resistance observed by Schuster and Stewart* on a magnetized copper wire.

31. The following series of experiments serves to show that the influence of the magnetizing is demonstrable even when accompanied by considerable influences of temperature. On making certain simplifying assumptions, it is easy to ascertain the law according to which the resistance in the circuit of the principal current changes with the time through lateral radiation of the spiral. If now, in addition to this, we carry out a longer series of alternate determinations of w_0 and w , and note in each the time t , we obtain by the numbers w_0 the constants of that law. If we then construct the curve representing the law (it is in general a transcendental) and compare it with the curve deduced from observations, we find different ordinates for the abscissæ corresponding to the times of the determination of the quantities w . These differences refer to the magnetization. I give a series of experiments of this kind with the Gramme machine and the wire F_3 ; w_0, w_1, w_2 are the resistances for $p=0, p=400$, and $p=800$.

t .	w_0 observed.	w_1 observed.	w_0 calculated.	w_2 observed.	w_0 calculated.	$w_1 - w_0$.
h m						
2 50	0.3285					
3 7		0.3294	0.3299			-0.0005
13	0.3315					
17	0.3326					
20		0.3332	0.3339			-0.0007
22		0.3338	0.3348			-0.0010
27				0.3346	0.3381	$w_2 - w_0$
34	0.3416					-0.0035

For $\frac{w_1 - w_0}{w_0} = \delta_1$ we find, according to the above, three values of ascending magnitude, for δ_2 one only, namely:—

$$\delta_1 = -0.0015, \quad -0.0021, \quad -0.0030;$$

therefore, mean,

$$\delta_1 = -0.0022; \text{ and } \delta_2 = -0.0106.$$

It is of course presupposed here that the discontinuities produced by the variation of p are masked by the residual thermal action.

The value of δ_2 here should agree with the value of δ in 29; for H, M, n have the same values. But it is more than double

* Phil. Mag. [4] xlviii. p. 535 (1874); Pogg. Ann. cliii. p. 205.

as great. Fig. 1 (Pl. I.) gives a part of the observed and the calculated curves.

§ 5. Hand in hand with the experiments on the influence of magnetization went experiments on the *extra currents which are generated by every current in iron*. I forbear to communicate these experiments in detail, since in their essential results they agree with those of MM. Herwig and Streintz. Quantitative statements, however, on the intensity of the extra currents, in any comparable measure, on the part of the latter observer we have none; and Herwig only states that once a diminution of the resistance of 0.0011 to 0.0014 would have corresponded to the deflection of the galvanometer-needle obtained after the opening of the principal current, if the current had persisted. As in general I took no account of an agitation of the wires, I mostly obtained extra currents of longer duration, by which I attained the possibility of sometimes carrying out very exact resistance-measurements of the above sort. The values which I obtained are, in part, by no means inconsiderably *higher* than Herwig's above mentioned. The signs of these values were, with few exceptions (to be explained by extraneous influences) constantly such as to show that *on the closing of the principal current the standard resistance must have been heightened, while on its opening that resistance must have become less*. Further, I find (as Herwig did) the deflections with *steel* less than with *iron*; but if I follow up the rate of each (which with iron, especially annealed wires, is much quicker than with steel), I find for the *integral currents* corresponding to the deflections *values not essentially different*.

Now I obtained far more intense extra currents when I led the current through magnetized iron or steel wires, although their direction was constantly the same as if the wire had not been magnetic. In those cases in which the definitive resistance was increased by magnetizing, a still greater resistance always corresponded to the extra current. This made itself recognizable by a deflection towards the side of the greater resistances, following after the bridge had been equalized for the unmagnetic condition of the wire, which deflection was greater than had corresponded to the permanent deviation of the needle; and this latter also, after the oscillations of the needle had long ceased, diminished, in most cases slowly, a little more, till it took the value given in the Tables of § 4. But even in those cases in which the resistance in the magnetic state was less, the deflection constantly took place first toward the side of the greater resistances. I do not give numerical data, because a similar diversity prevails as in the

numbers of § 4. The intensity of the extra currents increases with the amount of the magnetizing force, at the commencement more quickly, afterwards (*i. e.* with high magnetizings) more slowly than it.

It is here presupposed that the principal current is not closed till some time after the closing of the magnetizing one. If it be closed shortly after or simultaneously with that current, the phenomena become very irregular. They are totally changed when the principal current is closed first, and then the magnetizing one. The closing-currents are then feebler by far; but if accessible to observation, their direction is found to be the opposite; that is, they correspond to a diminution of resistance. Sometimes a whole series of oscillating extra currents is observed.

§ 6. The experiments communicated in § 4 appear to me adapted to mediate between the results obtained by previous observers relating to this subject. Indeed nearly all of these, however divergent they may seem, reappear in my results, and receive their provisional explanation by the variety of the circumstances under which they were gained. Beetz found under all circumstances an increase of the resistance; but he appears to have always employed very powerful magnetizing forces, and to have operated only with iron and not with steel rods; and under these two conditions I also constantly obtained positive values of δ . Stewart and Schuster observed in a magnetized copper wire, when the magnetizing force was great, a diminution of the resistance; it behaved, then, like my steel wires. Unfortunately, it is not stated whether the wire consisted of pure (diamagnetic) copper or of copper containing iron (paramagnetic), as commercial copper usually does. Edlund and Mousson did not get any alteration of the resistance by magnetizing, although the accuracy of their measurements was not essentially inferior to that of Thomson's. In my Tables also some are found which give for δ the value 0. Lastly, Adams published in the 'Proceedings of the Royal Society'* a preliminary communication from H. Tomlinson (but I have in vain sought the full paper in the 'Transactions'). According to these data, which afford extremely few fixed points, in hard steel the magnetizing has for its consequence a diminution, in iron and soft steel an increase of the resistance. This is in complete accordance with my statements, if it be admitted that the magnetization applied by Tomlinson was always considerable. This is probable from the enormous magnitude of the numbers which I designate

* June 17, 1875.

by δ . My values lie between the limits

$$-0.0186 \text{ and } +0.0281^*;$$

Tomlinson's values between

$$-0.06 \quad +0.04 (?).$$

Subjoining the limiting value from the experiments of Beetz,
 $+0.0006,$

with which Chwolson's exactly, and Thomson's in some measure, agree, we see that my numbers hold the middle place.

Tomlinson moreover appears to have carried out the magnetizings while the principal current was closed. I have mentioned that in such cases an extra current arises, corresponding to a diminution of the resistance. Now, since these extra currents in hard steel often pass very slowly, perhaps a part of the 6 per cent. decrease might result from this, and of the 1-4 per cent. increase in soft wires we might deduct a part for the heating acknowledged by Tomlinson himself.

[To be continued.]

II. *On Unilateral Conductivity in Tourmaline Crystals.*

By PROFESSOR S. P. THOMPSON and DR. O. J. LODGE†.

IN thinking of the possible physical conditions of structure which might permit an explanation of the phenomena of pyroelectricity of the tourmaline and other crystals, an hypothesis suggested itself independently to each of the present writers. At the Glasgow Meeting of the Association in 1876, a paper was read by the second-named of them, upon a Mechanical Model illustrating the phenomena of electric currents‡. The physical illustrations there given of the relation of electromotive force and resistance to the particles of matter in a conducting circuit led the way to the suggestion that the internal polarization of each particle of the crystal, which had been assumed by Sir William Thomson as a sufficient cause of the phenomena of pyroelectricity, might become explicable if it could be shown that such bodies as possessed pyroelectric properties possessed also a unilateral conductivity for electricity.

The term unilateral conductivity is defined as follows:—Let a certain direction from a point A to a point B in a homogeneous substance be considered. Then if it is found that the

* The value -0.03 has been given by only one experiment with a steel wire, $Mn : H$ being very small and the absolute value of H very great.

† Read before Section A of the British Association at Dublin, September 1878. Communicated by the Authors.

‡ See Phil. Mag. Nov. and Dec. Suppl. 1876.

resistance to the passage of electricity (or heat) is greater or less when the flow is in the sense AB than it is when in the sense BA, such a substance possesses a unilateral conductivity for electricity (or heat, as the case may be) in the given direction. Some apparent cases of unilateral conductivity for electricity had been described by Dr. A. Schuster (*vide* Brit. Assoc. Rep. 1874). It was to be expected that a phenomenon of unequal heating, the analogue of the unequal electrification of the tourmaline when warmed, would be found. It was also imagined, by a reversal of the known phenomena of pyroelectricity, that a pyroelectric crystal when electrified from without might have its ends unequally warmed. If the development of opposite electrical states at the two ends, and the establishment of a difference of potential between them, were a result of a unilateral conductivity, all the analogies of the conduction of heat and electricity pointed to the probability that the tourmaline would be found to possess a unilateral conductivity for heat also.

The first named of the authors, therefore, proposed the following experiment. Let a slice be cut from a tourmaline crystal having its two faces principal planes of section of the crystal, and therefore containing the crystallographic, optic, and pyroelectric axis. Let the slice be covered with wax, and let it be warmed by a hot wire inserted in a central hole after the method of De Sénarmont. The tourmaline we know to be a negative uniaxial crystal; and the isothermal line marked out by the melting of the wax will be an ellipse having its minor axis along the crystallographic (and optic and pyroelectric) axis. If the crystal, however, possess unilateral conductivity for heat, the isothermal lines will be no longer symmetrical about the point of application of heat, but will be displaced along the crystallographic (and optic and pyroelectric) axis toward one extremity. We therefore, as a preliminary trial, procured such a slice of tourmaline (which we will call tourmaline "A") from Mr. Ahrens. It was roughly circular, of about 2 millims. thickness, and measured along the axis 25·3 millims., across 25·6 millims. Experiment proved that when the point of a hot silver wire was introduced into the central hole, the isothermal line bounding the melted area possessed the form of a distorted ellipse, always displaced toward the analogous pole of the crystal. Two series of measurements were made:—one, of the areas marked out by the melting of the wax; the other, using Meusel's double iodide of copper and mercury, which changes at 94° (*circa*) to a black tint. This method gave isothermals of a higher temperature than the wax. The extremities of

the minor axis were scratched with a needle upon the surface, and afterwards measured. The following is the series of measurements made :—

CRYSTAL "A."

Measurements of Semiaxes minor of Isothermal Curves.

Series 1. (Wax.)

	Semiaxis -.	Semiaxis +.		
(a)	2.5	3.1	or	100 : 124
(b)	3.1	4.4		100 : 142
(c)	4.3	5.9		100 : 137
(d)	5.6	7.2		100 : 129
(e)	6.7	8.4		100 : 124
(f)	10	12.8		100 : 128

Series 2. (Double Iodide of Cu and Hg.)

(g)	2.0	2.5	or	100 : 125
(h)	3.1	4.2		100 : 135
(i)	3.9	5.1		100 : 130
(k)	5.9	6.9		100 : 117

Mean 100 : 129.1

These results differ somewhat widely from the mean figure ; yet it is significant that they all tend in the same direction. It is to be remarked that in all cases these experiments were made *quickly*, and that in no one case was a condition of thermal equilibrium established in the crystal between the central gain of heat and the loss by radiation &c. on the outer edges. The result then appeared to be that *in a crystal which was getting warmer* the heat flowed more easily *towards* the analogous pole than away from it.

It is clear that the isothermal curve produced thus will be one possessing geometrical discontinuity ; for the resolved parts of the radii from the centre of heating parallel to the axis will on one side of the centre be lengthened, and on the other side reduced, in a ratio dependent on the unilateral coefficient of conductivity. Hence the curve will consist of two semiellipses having a common major axis, but having different semi-minor axes in the ratio given above, viz. on the average 1.3. These results were obtained in July 1877. They were not published at the time, because it was desired to obtain further confirmation with other specimens of tourmaline. No suitable specimens, however, could be met with ; and the matter rested at this point for some months*.

* Another tourmaline, a transparent green crystal, obtained from Laurent of Paris, has since afforded equally significant results—the isother-

It then occurred to the authors that it would be worth while to measure the flux of heat and of electricity across a thin wall of tourmaline, the crystal being cut for that purpose at right angles to its crystallographic axis. For some months no suitable crystal could be obtained; but eventually, by the kindness of Professor N. Story Maskelyne, this difficulty was removed, and a slice of opaque black tourmaline from the Ural Mountains, which we will speak of as tourmaline "B," was placed at our disposal. Its dimensions were 35.5 millims. length by 24.4 millims. width; and it varied in thickness from 2.5 to 2.14 millims.

Two methods of measuring the flow of heat through such a slice were suggested. One method, applicable only to a slice at a constant temperature, is described, and the mathematical solution of the case is given, in the *Philosophical Magazine* for February 1878, by the second-named of the authors, under the title "On a Method of Measuring the Absolute Thermal Conductivities of Crystals" &c. The opportunity of applying this method to the tourmaline has not, however, arrived.

A second and simpler method of experimenting, and giving results of qualitative value only, was suggested and carried out by the first-named author. It involves the use of a sort of reversible contact-thermometer. The crystal-slice was fixed between two portions of a glass tube. In one of these a weighed quantity of mercury was placed and a thermometer. Into the other steam was blown, so that heat passed upward through the crystal and warmed the mercury. After taking a measurement in this position the apparatus was inverted, the mercury being placed in the other tube, and the steam directed into the tube formerly serving as the calorimeter. The glass tube chosen was about 20 millims. in internal diameter, and about 1 millim. thick. It was divided into two parts, the edges fused and thickened and then ground flat, and *very slightly* smeared with a mixture of resin and Canada balsam. The crystal slice was placed between them, and they were bound together with strips of black caoutchouc. This joint was perfectly watertight. After a number of experiments in which the heat was made to pass through the crystal first in one direction, then in the other, the crystal was taken out, reversed in position between the two glass tubes, again secured in its place, and a fresh series of experiments were made. The condition of the crystal, as far as could be ascer-

mal curves being obtained for low temperatures by the melting of films of cocoa-butter, and for higher temperatures by films of solid paraffins of definite fusing-point.

tained, was the same after the series of experiments as before. The same weight of mercury (221 grms.) was employed throughout. The source of heat (the steam) was applied below the crystal in every case. The original temperature of the mercury having been read off on the thermometer, the heat was applied until the temperature rose through a given range—in the first experiment 50 degrees C., in the subsequent ones 40 degrees C. Mercury was employed as being more convenient in several ways, though a water calorimeter would probably be more accurate. The results of nineteen experiments, arranged in two series, are appended in a tabular form. In this Table the faces of the crystal-slice are denominated α and β . It was found by experiment that the face β corresponded to the analogous, α to the antilogous pole of the original crystal.

Experiment-series.	Range. T—t.	Time occupied and direction of flow.		Time-ratio.	Initial temperatures. C.	Which operation first.
		α to β .	β to α .			
I., II.	50	m s 3 30	m s 5 0	100 : 142	19° and 22°	β to α
III., IV.	50	4 15	6 0	100 : 141	22·25 „ 23	β „ α
V., VI.	50	3 40	3 30	100 : 94·5	15·5 „ 20	β „ α
VII.	Accident water boiled away.					
VIII., IX.	40	2 30	3 00	100 : 120	17·5 „ 21·25	α „ β
X., XI.	40	2 30	2 40	100 : 107	18·75 „ 19·5	α „ β
Crystal then reversed between tubes.						
I', II'.	Accidental escape of steam around calorimeter.					
III., IV.	40	2 45	3 15	100 : 118	25·5 „ 27	β „ α
V., VI.	40	3 00	3 25	100 : 108·3	22·5 „ 26·25	α „ β
VII., VIII.	40	2 35	3 25	100 : 125	24·75 „ 26·25	α „ β
Mean time-ratio				100 : 119 (α to β) (β to α)		

It would appear therefore, by this method of experiment, that the ratio of the two conductivities in opposite directions through the crystal while it was becoming hotter was roughly as 100 to 119. The more rapid flow is toward that pole which, when the crystal is warmed, becomes positively electrical.

Electrical Conductivity.

It only remains to add, that the second-named of the authors has made a few preliminary experiments with the view of detecting any unilateral conductivity of tourmaline for *electricity*. These experiments were made with the same slice of tourmaline, “B,” as had been employed in the last-mentioned heat experiments—a crystal in which the faces were normal to the

crystallographic axis. The crystal was heated to 100° Centigrade in a steam-bath. A 5-microfarad condenser was charged through the crystal with 10 or 12 Daniell's cells for a minute ; and then the condenser was discharged through a sensitive astatic Thomson galvanometer of 7000 ohms resistance. The conductivity of the tourmaline was so slight that the motion of the spot of light was only about two inches. The limit of swing was accurately observed, and then the operation repeated with the tourmaline (electrically) reversed. This was done again and again alternately in opposite directions.

When the temperature of the tourmaline was *rising*, a distinct difference was perceived between the to-and-fro discharge. Also when it was *falling* there was a difference, in the other direction. (These effects are of course due to that which is ordinarily termed electromotive force in the warming or cooling crystal.) But with a perfectly steady temperature, which was only attained after some hours, not the slightest difference could be perceived. But before being satisfied with this imperfect and negative result, we should wish to use a battery of very much higher electromotive force (say 1000 cells), and, if possible, heat the tourmaline above 100° , so as to increase its conductivity, which at 100° is slight.

Note added, May 1879.

Since the above was communicated to the British Association, a large number of attempts have been made to detect a different conductivity for electricity from α to β to that from β to α when the temperature of the crystal was uniform. Numerous leakage methods have been employed, the most powerful having been one with a quadrant electrometer and a dry pile. Another slice, larger in area and only a millimetre thick, has been also used ; and the crystals have been tried when cold, when at 100° , and when heated up to 300° in an air-bath ; but although a small difference was many times observed, indicating a better conductivity in one direction than in the opposite, yet it always decreased, and usually vanished when excessive care was taken. It was very difficult to be quite sure that the temperature of the crystal when hot was not slightly changing ; and the electromotive force due to such change of temperature was perceptible by the methods employed when its direct effect on a quadrant electrometer was inappreciable ; and when the crystal is cold its conductivity is exceedingly small. It is intended, however, to try the best method once more, and more carefully ; and also it is intended to see if no difference can be perceived between the *specific*

inductive capacity of a cold crystal in opposite directions along its axis.

So far, then, as experiment has at present gone, the results for the case of electricity have been altogether negative. For the case of heat, distinct results have been obtained; but it is to be remembered that a rising temperature has always been used, and no experiments on the conductivity for *heat* have been at present made with the temperature constant: it is probable that, when they are, negative results will be also obtained corresponding with the negative results for electricity. We have roughly repeated the heat experiments with a *falling* temperature, and have obtained the ratio of conductivities inverted.

Note by the second-named Author.

It thus appears that our original hypothesis with regard to the cause of the internal polarity of the particles of pyroelectric crystals, at any rate in the form in which I put it forward in section 26 of the paper “On a Mechanical Illustration of Thermoelectric Phenomena” (Phil. Mag. Dec. Suppl. 1876), has not been confirmed by experiment.

But instead of this, the important result has been obtained by Professor Thompson that, in a pyroelectric crystal whose temperature is rising, Heat flows more easily *with* the Electricity (*i. e.* from the antilogous towards the analogous pole) than it does against the electricity. This “convection of Heat by Electricity” has an apparent analogy with the effect predicted and verified by Sir William Thomson in unequally heated metals (Bakerian Lecture, 1856), and which might equally well be called *the convection of Electricity by Heat*; and it must have an interesting bearing on the theory of Prof. Kohlrausch concerning *Thermo-electricity and Heat-conduction*, set forth in Pogg. Ann. vol. clvi. p. 601.

As to the original hypothesis, I am unable to give it quite up even now. For though I have no faith in unilateral conductivity in isotropic conductors like metals (upsetting as such a thing would be to Ohm’s law, which has been accurately verified), yet in hemihedral crystals it did seem very possible that greater resistance should be offered to the motion of electricity in one direction through them than in the other, just as an ear of rye-grass is rougher one way than the other—that there should in fact be something analogous to *barbs*, or *valves*, so that when a non-directional disturbance (like a uniform rise of temperature) was imparted to the crystal the electricity should be urged from α to β more strongly than in the reverse sense. Professor Maskelyne once told me that some crystals

were differently *hard* in opposite directions, so that they were more easily scratched in the sense AB than BA ; and if they possess one such unilateral property, they must surely have others. One surely ought to expect that a current driven through a pyroelectric crystal from analogous to antilogous pole would heat it, and that a current in the reverse direction would cool it, or heat it less. If, however, as I begin to fear, this is a wrong scent, I should be very grateful to any one who will kindly point the fact out.—OLIVER J. LODGE.

III. On the *Fracture of Colloids*. By FREDERICK GUTHRIE*.

[Plates II. & III.]

§ 1. A PROMINENT property with regard to solid colloids is that they have neither crystalline form nor planes of cleavage. When such a body is broken it offers the so-called conchoidal fracture. An agglomeration of crystals may present in mass the conchoidal fracture usually associated with colloids. This is the case with granite, and eminently so with basalt, all of whose constituents are crystalline. When the solid has resulted from the intersolution of two or more crystalloids it may, like glass, present the colloidal fracture in a most marked manner. And, indeed, even single crystals themselves are often subcolloidal in fracture ; that is, conchoidal fracture accompanies the crystalline. This state is shown by the diamond, sugar-candy, quartz, &c.

I assume here that every cohesionally homogeneous mass of solid matter will break conchoidally when subjected to pressure sufficient to cause fracture.

Experiments.

§ 2. The cracking of a glass plate by pressure offers no special features of interest. A round plate placed on a thick soft cloth and pressed in the centre by a round cork cracks radially ; the cracks are generally slightly curved. Fig. 1 shows two examples of fracture of crown glass by pressure in the centre. Similarly, if a round sheet of glass placed on a thick soft cloth be pressed down at its circumference by means of cardboard rings, the same class of crack is produced ; for, indeed, the two conditions are essentially identical.

§ 3. The internal strain caused by difference of temperature causes fracture of great regularity and beauty. It rarely happens that a sheet of glass of any shape breaks into only two pieces when heated. If a circular piece of "crown"

* Read before the Physical Society, March 22, 1879.

glass, about $3\frac{1}{2}$ inches in diameter, be loosely balanced horizontally between the lips of a wooden clip and brought with its centre over an air-gas burner so that the top of the flame is about an inch below the glass, the latter almost invariably cracks at least into three pieces; and when the pieces are three in number they as invariably have the form shown in fig. 2.

The remarkable symmetry of each of these, and their similarity to one another, show that the shape is not an accident of the glass. The constant features are (1) that the two main cracks join before reaching the circumference, (2) that there are in each crack, reckoned from this confluence, three concavities towards the centre of the circle, the first being nearly straight, (3) that there is a little kick given by the crack as it leaves.

Out of sixty-four specimens of fracture produced under these conditions ten showed this species of two-crack fracture. The shapes of the cracks are perfectly similar to those given; and the symmetry is sometimes such that the side pieces may replace one another after inversion so perfectly that it is scarcely possible to tell that they are misplaced.

§ 4. The same method of heating may result in the production of a great variety of forms; but they are all derived from the above type. In fig. 3 are shown a few of the more simple.

The three-crack figures (*a, a*) are about as frequently formed as the two-crack figures. I find eight of the three-crack out of sixty-four similarly treated specimens. Perfectly similar forms were got when the plate was laid on a retort-ring or supported on three corks and heated in the same manner. A special series of experiments was moreover made to see if the position of the clip had any influence upon the attitude of the crack. In figures 2 and 3 the mark † shows where the plate was held. As to the effect of the position in the original sheet of glass of the pieces experimented on, as determining the attitude of the axis of cracking, the following examination was made. Six pieces having been marked as they lay in the sheet, forming a radial band, were cut out and heated as above. The apex of crackage always appeared somewhere on the semicircle which was towards the centre of the sheet, but varied in this semicircle so considerably that it is at present doubtful whether the position in the sheet influences the crack-axis. Fig. 4 shows the amount of variation; the point of the arrow represents the apex of the crack-curve. The lower figure represents the original sheet and the positions of the several pieces in it.

§ 5. If the sheet of glass be made very much larger, or the flame smaller and more pointed, another alteration of the crack-figure ensues. The apical point of the previous figures advances into the sheet; and this is followed by a fan-like cracking of the glass between the apex and the still nearest circumference. In fig. 4, *a* shows the cracking of a plate of crown glass, 9 inches in diameter, over an air-gas burner; *b* is a $5\frac{1}{2}$ -inch-diameter plate similarly treated. In *c* we have a plate of crown glass, 3 inches in diameter, which was laid on a cloth and heated from above by a fine blowpipe-flame. If we conceive what was before called the apical point to reach the centre, the heat fracture would become approximately the central-pressure fracture, namely radial.

§ 6. A piece of plate-glass $\frac{1}{4}$ inch thick and a little over 7 inches in diameter, cracked when heated in the centre over an air-gas burner, as shown in fig. 5 *a*. A piece of "sheet-glass" (Chance's), 3 inches in diameter, cracked as shown in *b*. A slab of resin $\frac{1}{2}$ inch in thickness and $3\frac{1}{2}$ inches in diameter, heated in the centre by a jet of low-pressure steam, gave the fissures shown in *c*. Square porcelain tiles cracked nearly straight across in one crack.

§ 7. Pieces of crown glass of various shape were next examined, with the result which declares itself in fig. 6. The pieces were supported at the point marked *c*, and the flame applied below the point marked †.

The figure 6 shows that the same general type is preserved. It instructs us that the apical point seeks one of the nearest points of the circumference.

§ 8. Experiments were next made for the purpose of ascertaining under what circumstances, if at all, a crack could cross a crack. A circular plate of crown glass was cut by the diamond in concentric rings, and the crack was made to pass, by tapping, completely through the thickness of the glass, around the whole circumference. Such divided glass on being heated in the centre over an air-gas burner cracked according to the same type as before. Sometimes the heat-crack would run across the diamond-crack, as though the latter had no existence. Sometimes the heat-crack would follow, and, as it were, adopt the diamond-crack, and then break off. In the latter of such cases the inner circle may be suffering a three-crack fracture, while the outer ring exhibits only a two-crack fracture on the converse. Fig. 7 (*a*) exhibits the former circumstance, fig. 7 (*b*) shows the influence of a greater number of concentric cracks.

§ 9. The heating of the central part of a circular plate

should give the same crack-figure as the cooling of the circumference ; and, as a matter of experiment, it is found that the figures are very similar. In order to cool the circumference of a heated circular plate with some approach to uniformity, an annular trough was constructed by cementing concentrically two glass cylindrical vessels of the same depth but different diameters, one inside the other, and over-filling with mercury, so that the convex surface of the metal projected. The glass was held in a wooden clip widely stretched, so that the axis of the clip being vertical the plate was horizontal. Held, when uniformly hot, immediately above the mercury, it was let drop and pressed down in the middle by a piece of wood. The fracture is in this case instructive ; for while in fig. 8 (*a*) the old type got by heating the centre is resumed in *b* and *c*, the fracture is either influenced or even accompanied by the circular fracture along or near the line of greatest temperature-difference.

§ 10. It is clear that heating in the central regions should produce a similar fracture to that brought about by cooling around the circumference, and cooling at the centre a similar fracture to that caused by heating the circumference.

On heating a circular plate at the circumference by means of a " rose " air-gas burner, it breaks with far greater violence than when fracture is produced by central heating. The parts are scattered at least three times as far in the former as in the latter case. The form of the fracture is essentially radial ; but the fragments, even when the primitive type is widely departed from, present wonderful symmetry. A noticeable point in this fashion of fracture is the invariable appearance of two pieces on opposite sides of the centre whose form is approximately rectangular ; that is, their sides are half-cords instead of radii. This form suggests that there are two chief centres of maximum fracture, and that the bounding radii of the two systems are parallel. In fig. 9 the pieces marked *a* represent these singular pieces. Out of seven plates which have been broken in this way, there is not one in which this feature is absent.

§ 11. As to cooling a hot plate in the centre, I find such extreme difficulty in reproducing the inverse conditions of heating a hot plate at the circumference that I have rarely succeeded in reproducing the same type of fracture.

Also it is seldom the case that a sheet of glass cracks during heating at its edge. More frequently a sheet of glass which has been heated at its edge cracks when cooling. The crack then appears to follow that isothermal line along which there

is the greatest difference of temperature at right angles to the line.

Remarks.

§ 12. About the fracture by mechanical strain it may appear to satisfy many that the lines of fracture are perpendicular to that resultant of the pressure which lies in the plane fractured. A tear in a sheet of paper is at right angles to the two opposing pressures, or rather to their resultants at the point yielding.

What is a crack? Which are its beginning and end? In only one of the above-cited experiments can the growth of a crack be followed. In § 11 when a plate heated at the edge has refused to crack while being heated but cracks on cooling, the crack is seen to extend from the edge of the plate inwards, following, generally speaking, a semicircular path, but sometimes curiously modified towards the centre of the curve.

A crack is neither a line of least cohesion nor a line of greatest strain. Nor is it a line where $\frac{c}{s}$ has a series of minimum values. The more perfectly elastic a medium is, the more fully does the crack resemble a flash of lightning or wisely laid railway-line, and the more it departs from the river-course or the descent of a globule of mercury down an inclined but undulating surface. Its path is the curve whose course is determined by the integral of $\frac{c}{s}$ being a minimum.

The sudden splitting through of the solid æther by the electric discharge furnishes us with figures by no means remotely resembling those of the fracture of glass. Even or rather especially the forms of fig. 4 remind us of this.

As to the typical form in fig. 2, it has been suggested by my brother, Mr. Charles Guthrie, that this form is a compromise between the circular line of fracture along some isothermal line where the difference of temperature is greatest, or rather the difference of expansion is greatest, with the three lines of relief which would be radii at angles of 120° with one another.

This is a very suggestive hint; but, for reasons sufficiently apparent from the foregoing, it is insufficient.

IV. *On a neglected Principle that may be employed in Earthquake Measurements.* By JOHN PERRY and W. E. AYRTON, Professors in the Imperial College of Engineering, Japan*.

[Plate IV.]

SPECULATIONS regarding the internal constitution of the earth have interested philosophers for many years. For a long time it was considered that our globe consisted of a thin solid shell containing a fluid core : but Hopkins, who was one of the first to investigate the subject on correct principles, showed that this shell must be from 800 to 1000 miles in thickness; and still more recently Sir Wm. Thomson has proved that the apparent absence of elastic tides in the earth's surface leads to the conclusion that the average rigidity of the earth is greater than that of glass, and possibly even greater than that of steel. We do not on the present occasion propose to consider whether the state in which the internal part of the earth exists is like any state of matter with which we are acquainted ; but this is, of course, a subject well worthy of very careful investigation.

It is probable that the earth was once in a molten condition and that it now is cooling ; so that the shrinking resulting from this cooling must develop vast internal forces, producing strains, or deformations, of great magnitude. Other powerful forces, too, brought into existence by water being suddenly changed into steam on entering a hot cavity, by the sudden chemical combination of gases, or possibly by elastic tides in the earth's substance produced by the joint attractions of the sun and moon—all tend to cause disturbances and ruptures which are brought vividly to our notice by volcanos and earthquakes.

An earthquake has been defined by Mr. Mallet as “the transit of a wave, or waves, of elastic compression in any direction, from vertically upwards to horizontally, in any azimuth, through the crust and surface of the earth, from any centre of impulse or from more than one, and which may be attended with sound and tidal waves dependent upon the impulse and upon circumstances of position as to sea and land.” If we could only read the earthquake message rightly, we should learn all about the deformation going on in the earth's crust ; for there is no doubt that the nature of the stresses and strains, and every condition of the rocks at the origin of motion, all give their character to the earthquake vibrations. It must be remembered, however, that the message before it

* Communicated by the Authors, having been read before the Asiatic Society of Japan on the 23rd of May, 1877.

reaches us is much modified by the media through which it has been transmitted; and, again, since there is a great want of continuity at the surface of the earth, very important modifications are introduced by surface conditions: for example, ranges of mountains are well known to reflect earthquake vibrations in a marked manner; and veins of good conducting rock, by transmitting the vibrations more rapidly than less conducting veins, set up transverse vibrations.

Professor Palmieri and others have invented instruments which record the date of the vibration, and give rough ideas of the direction of propagation of the earthquake-waves, together with what is called the *strength* of the vibration.

Mr. Mallet, whose wide experience on the subject of earthquakes has necessarily caused his writing to be regarded with great respect, describes the object of Professor Palmieri's instrument as follows:—"By means of this apparatus the time of the first shock is recorded, as well as the interval between the shocks, and the duration of each; their direction, whether vertical or horizontal, is given, as also the maximum of intensity." He further says, however:—"It is not my intention here to offer any criticism as to the construction or performance of this instrument, the rather as I must confess I do not quite share the high opinion of its inventor as to the certainty or exactitude of its indications." And this opinion of Mr. Mallet with regard to Professor Palmieri's instruments is ours with regard to all the seismoscopes of which we have read any descriptions. Indeed it is well known that the instruments hitherto invented have not satisfied even the modest hopes of their inventors; whereas, even if these hopes had been fulfilled, we should still hardly have made a step in this new science.

A simple form of seismoscope, but by no means a perfect one, would be a lamp suspended from a ceiling by a spiral spring, of such a strength that the period of vibration of the lamp in a vertical direction was nearly the same as that for its vibrations when swinging as a pendulum. The vibrations of such a lamp during an earthquake would contain motions due to the motion of its point of suspension; and an experienced observer would be able during a shock, or very soon after it, to tell the direction and strength of the shock with much more accuracy than with any of the instruments previously described. This lamp seismoscope, however, possesses the defects of all slowly vibrating bodies: the main vibration of the lamp is (as we shall presently show) executed in its ordinary periodic time; and the lengths of its swings depend on many other things besides the strength of the shocks, which would show themselves as small perturbations in the motion of the lamp.

If, however, instead of actually observing the lamp we merely get a record of its greatest swing, then very little information could be obtained of the strength of the shocks; for the great or small deflection of a slowly vibrating pendulum during an earthquake will depend on whether the period of the earthquake is or is not some submultiple of the period of the pendulum; so that considerable mathematical knowledge and much time would be requisite to deduce from the comparatively small ripples on the larger vibrations the nature of the earthquake. In addition, as the length of the swings of the lamp will generally be much greater than the earthquake vibrations, they will, if recorded on paper, require a very large recording apparatus.

We now proceed to the principle which is to enable us to record an earthquake-message. It must be evident that the message can only be correctly recorded when we have obtained the complete motion at every instant of time during the earthquake of a large portion of the rocky crust of the earth. Any point P in the solid earth has a certain position, a certain velocity, and a certain acceleration in a certain direction in any instant of time during an earthquake; and if we know these elements we are said to know the motion of P. Now we have a complete record of an earthquake when we know the motions of all points P affected by the earthquake; and if the earth were rigid, this could be derived from a knowledge of the motion of three of its points not in the same straight line. Still, although the earth is not rigid, and although the conditions of motion, of different parts of an elastic non-homogeneous solid are very complicated, we may say that the important character of an earthquake, its origin and the media through which it has travelled, as well as its rate of motion, are recorded, and may perhaps be easily deduced from the known motions of three well affected points in the solid earth. Believing this to be the case, and seeing how important it is to the whole science of terrestrial physics that the earthquake-message should be read, we have been led to investigate mathematically the motion, during an earthquake, of a body attached to the earth by springs. And we have come to the conclusion that the centre of mass of a body fastened by means of springs inside a metal box rigidly attached to the earth, has in certain cases motions with respect to the box itself which in miniature, with great exactitude, represent the motions of a point of the box during the earthquake—this result being truly obtained when the springs are exceedingly strong, so that the motion of the mass relatively to the box is exceedingly small, and practically obtained when the springs are so strong that the

vibrations possible for the mass when there is no earthquake are several times quicker than the earthquake vibrations themselves; that when the springs, however, are weak, the motion of the mass relatively to the box in no way represents the absolute motion of the box itself; but that the introduction of friction, although it diminishes the accuracy of observation of regular vibratory earthquakes made by means of very rapidly vibrating springs, makes it possible to get an approximation to accuracy even with slowly vibrating springs, and is always desirable when the earthquake vibration is irregular and intermittent. In fact, in order that the motion relatively to the box of the centre of mass of the body supported in it may accurately represent the real motion of a point of the box itself, it is necessary that the mass be large, and the springs supporting it so strong that its natural time of vibration shall be about five times as fast as that of the earthquake itself, supposing no friction be employed beyond that necessarily introduced by the mechanism of the recording apparatus: or a much larger mass may be suspended by weaker springs if the chamber be filled with water, or some oily or tarry compound which will introduce the necessary amount of friction.

Let A B (Pl. IV. fig. 1) be a rigid box firmly attached to the earth; M is a large mass acted on by two horizontal springs, and subjected to no forces except those introduced by the springs, its weight, for example, being neglected. When both the box and M are at rest, let their centres coincide at the point C.

First let the box be at rest, and let M be made to vibrate in a horizontal line passing through its centre, and let y be its distance at any time t from a point O fixed in space; then

$$\frac{d^2y}{dt^2} = -n^2(y - OC), \quad . \quad . \quad . \quad . \quad (1)$$

or

$$(y - OC) = P \cos (nt + Q);$$

where P is the amplitude, and where

$$n = \frac{2\pi}{T},$$

T being the periodic time.

Next let the box be in motion in a horizontal direction, and let z be the distance of its centre from the fixed point O at the time t ; then

$$\frac{d^2(y - z)}{dt^2} = \frac{d^2y}{dt^2} - \frac{d^2z}{dt^2} = -n^2(y - z) - \frac{d^2z}{dt^2},$$

from (1). If the velocity of the box is uniform,

$$\frac{d^2z}{dt^2} = 0;$$

therefore the relative motion of M about the centre of the box is a simple harmonic motion.

Let the box have a uniform horizontal acceleration a , then

$$\frac{d^2(y-z)}{dt^2} = -n^2 \left(y-z + \frac{a}{n^2} \right);$$

therefore the body M has a simple harmonic motion about a point at a distance $\frac{a}{n^2}$ behind the centre of the box.

Now, whatever be the forces acting on the box or the ball,

$$\frac{d^2(y-z)}{dt^2} = \frac{d^2y}{dt^2} - \frac{d^2z}{dt^2};$$

or the acceleration of the ball relative to the box equals the absolute acceleration of the ball minus that of the box.

Let M be resisted with a frictional force proportional to its mass and to its velocity relative to the box, let $2f$ be the frictional coefficient, and let the earthquake vibration be a regular harmonic motion about the fixed point O; then

$$\frac{d^2(y-z)}{dt^2} = -2f \frac{d(y-z)}{dt} - n^2(y-z) + n_1^2 A \cos(n_1 t + B),$$

where A is the amplitude of the earthquake vibration, and

$$n_1 = \frac{2\pi}{T_1},$$

T_1 being the periodic time of the earthquake vibration. If when the time is nought the box is at the limit of its swing, then B is nought, or

$$\frac{d^2(y-z)}{dt^2} = -2f \frac{d(y-z)}{dt} - n^2(y-z) + n_1^2 A \cos n_1 t;$$

from which, substituting x for $y-z$, we get

$$\frac{d^2x}{dt^2} = -2f \frac{dx}{dt} - n^2x + n_1^2 A \cos n_1 t$$

as the equation of relative motion of the centre of M. Now the maximum acceleration of the box is $n_1^2 A$, or $\frac{4\pi^2 A}{T_1^2}$; consequently, if this acceleration were constant, and if there were no friction impeding the motion of M, the mean position of the centre of M would be behind the centre of the box by a

distance

$$\frac{n_1^2 A}{n^2} \text{ or } \frac{4\pi^2 A}{\frac{T_1^2}{n^2}}.$$

Let this distance be numerically equal to E , then

$$\frac{d^2 x}{dt^2} + 2f \frac{dx}{dt} + n^2 x - n^2 E \cos n_1 t = 0.$$

Section A.

The first and at present the most important case to consider is when f is less than n^* .

The integral of this equation is

$$x = e^{-ft} D \cos(\sqrt{n^2 - f^2} t + F) - \frac{En^2 \cos\left(n_1 t + \tan^{-1} \frac{2n_1}{n_1^2 - n^2}\right)}{\sqrt{(n_1^2 - n^2)^2 + 4n_1^2 f^2}}. \quad (2)$$

For facility of calculation we assumed above that the box was at the limit of its swing when the time was nought. We must now make some assumption with regard to the initial position of M in the box. As the most important point to consider is whether M , by its motion relative to the box, correctly records the vibration of the box when this vibration in some way *suddenly alters* its character, we arbitrarily assume that, when the time is nought, M is at the limit of its swing in the *positive* direction—since we know that if the vibration of the box did not alter its character, and if M were previously correctly recording, then at time nought M would be at the limit of its swing in the *negative* direction.

When	$t = 0,$
let	$x = E,$
and	$\frac{dx}{dt} = 0.$

By substituting in (2) these values, we find

$$\cos F = \frac{E}{D} \left\{ 1 + \frac{n^2(n_1^2 - n^2)}{(n_1^2 - n^2)^2 + 4n_1^2 f^2} \right\}$$

$$D = \frac{En_1^2 n}{\sqrt{(n^2 - f^2)[(n_1^2 - n^2)^2 + 4n_1^2 f^2]}};$$

* This is the condition which allows M , when disturbed, to swing about its position of equilibrium with an infinite number of decreasing deflections right and left. As f increases, we see, on examining the first part of the integral, that the periodic time of M about its position of rest becomes longer and longer, and the swings of M diminish more rapidly in amplitude.

so that, given $n, n_1, 2f$, and A , we can find the position of M with respect to the centre of the box at every instant.

I. Let there be no friction impeding the motion of M in the box—that is, let f equal nought, then equation (2) becomes

$$x = \frac{En_1^2}{n_1^2 - n^2} \cos nt - \frac{En^2}{n_1^2 - n^2} \cos n_1 t,$$

a composition of two harmonic motions of different periods and amplitudes; or it may be expressed as

x = distance of centre of M from the centre of the box due to natural vibrations of the spring without earthquake.

— distance of the centre of M from the centre of the box due to earthquake motion, M being supposed to have no natural vibrations due to the springs.

Now if we want the relative motion of M to represent the earthquake vibration, we must have

$$\frac{En^2}{n_1^2 - n^2} \text{ many times greater than } \frac{En_1^2}{n_1^2 - n^2},$$

or

$$T_1 \text{ many times greater than } T.$$

For example, let the springs be so strong that

$$T_1 = 10T;$$

that is,

$$n = 10n_1;$$

then

$$x = -\frac{E}{99} \cos nt + \frac{100E}{99} \cos n_1 t;$$

or the vibrations of M due to the natural vibrations of the springs have an amplitude only $\frac{1}{100}$ th part of the vibrations of M which represent the earthquake. In fact M by its relative motion in the box merely records the earthquake vibrations, to a scale diminished nearly in the ratio of n^2 to n_1^2 , or as 100 to 1, and the natural vibrations of the spring are quite imperceptible.

If now we take the opposite case where the springs are weak, so that the natural vibrations of M are slower than the earthquake vibrations, we find, supposing

$$T = 3T_1,$$

or

$$n_1 = 3n,$$

$$x = \frac{9E}{8} \cos nt - \frac{E}{8} \cos n_1 t;$$

that is, the amplitude of the motion M relative to the box caused by the natural vibrations of the springs is nine times as great as that due to the earthquake vibrations.

II. Let

$$f = \frac{1}{2}n,$$

and let the springs be strong so that M has a natural vibration quicker than the earthquake vibration. For instance, let

$$T_1 = 10T,$$

or

$$n = 10n_1;$$

then, from (2), the general solution for f less than n , we find

$$x = e^{-5n_1 t} \frac{E}{86 \cdot 1} \cos(8 \cdot 66n_1 t + 90^\circ) - E \cos(n_1 t - 5^\circ 46').$$

Now when t is nought, the first term, which is due to the natural vibrations of M independent of the earthquake, is small compared with the second term, which is due to the earthquake itself; and, in addition, as t increases, the first term grows rapidly smaller; therefore we may say from the beginning x represents the position of M due to the earthquake only, and is independent of the natural vibrations of M . Now let the springs be weak, so that they have a natural vibration slower than the earthquake vibration. Let

$$T = 10T_1,$$

or

$$n_1 = 10n,$$

and

$$f = \frac{1}{2}n,$$

as before; then

$$x = e^{-\frac{n_1}{20} t} \times 1 \cdot 16 E \cos(1 \cdot 16 n_1 t + 80^\circ 5') - \frac{E}{96} \cos(n_1 t + 5^\circ 46').$$

At the beginning we see that the natural vibrations of M greatly preponderate, and that it is not until

$$t = \frac{T}{\pi} \log_e 111 \cdot 4$$

that the amplitude due to the natural vibrations becomes diminished to $\frac{E}{96}$. After this time the vibrations due to the earthquake begin to preponderate and eventually entirely mask the others, and the amplitude becomes $\frac{E}{96}$ —that is, a little greater than A or the amplitude of the earthquake vibration. It is interesting therefore to notice that, with a weak spring, using friction, although the vibrations of M do not represent

the earthquake vibrations at the beginning, they do after some time if the earthquake only lasts long enough, and continues to be exactly the same pure harmonic motion.

Section B.

Let the friction be such that

$$f=n;$$

then the solution of the differential equation becomes

$$x=e^{-ft}(G+Ht)-\frac{En^2\cos\left(n_1t+\tan^{-1}\frac{2n_1f}{n_1^2-n^2}\right)}{\sqrt{(n_1^2-n^2)^2+4n_1^2f^2}}.$$

Making

$$x=E,$$

and

$$\frac{dx}{dt}=0,$$

when

$$t=0,$$

we find

$$\frac{G}{E}=\frac{n_1^4+3n_1^2n^2}{(n_1^2+n^2)^2},$$

and

$$\frac{H}{E}=\frac{n_1^2n}{n_1^2+n^2}.$$

Let the springs be strong, and

$$n=10n_1,$$

then

$$x=e^{-10n_1t}E\left(\frac{301}{10201}+\frac{10}{101}n_1t\right)-\frac{100}{101}E\cos\left(n_1t-\tan^{-1}\frac{20}{99}\right).$$

If the springs are weak, and

$$n_1=10n,$$

then

$$x=e^{-\frac{n_1t}{10}}E\left(\frac{10300}{10201}+\frac{100}{101}n_1t\right)-\frac{E}{101}\cos\left(n_1t+\tan^{-1}\frac{20}{99}\right).$$

These results are of the same nature as before: with the strong springs we see, since e^{-10n_1t} is small compared with $\frac{100}{101}$, that x represents the displacement due to the earthquake only; with the weak springs, when t is small, the natural vibrations of M preponderate and mask the earthquake-effect; but as t increases, these vibrations become smaller relatively to those due to the earthquake, so that the weak springs will eventually record the earthquake if it only lasts long enough.

Section C.

Let

$$f > n;$$

then the general solution of the differential equation becomes

$$x = e^{-ft} D \cos(p\sqrt{f^2 - n^2}t + F) - \frac{En^2 \cos\left(n_1 t + \tan^{-1} \frac{2n_1 f}{n_1^2 - n^2}\right)}{\sqrt{(n_1^2 - n^2)^2 + 4n_1^2 f^2}};$$

as before, the values of D, E, and p must be determined from the character of the motion when t is nought.

When f is equal to or greater than n , then an examination of the first term of the solution shows that M has *not* a natural vibratory motion; but if deflected from its position of rest when there is no earthquake, it will gradually approach this position but never reach it.

Although, therefore, the first term of the above solution rapidly disappears (that is, the natural vibrations of the springs die away, whatever be the strength of the latter), still the application of recording apparatus, and the necessity that M shall reach its mean position in a reasonably short time after disturbance, have caused us to restrict ourselves to cases in which f is less than n^* .

At the commencement of section A, it was explained that the box and mass M were both assumed to be deflected from their positions of rest when the time equalled nought. It must now be observed that M was supposed deflected to the opposite side of the centre of the box to that towards which it would be deflected on the box receiving a shock; and the following investigation will show that that assumption really corresponded with a sudden change in the form of harmonic motion in accordance with which the box was moving, or what may be called a discontinuity in the motion of the box. For while we have proved that, with our original suppositions, the motion of the box was instantaneously recorded by the motion of M if the springs were strong, but that if they were weak the early vibrations were lost, and that it was only after some time, and then only provided the earthquake lasted long enough, that a record was left, we shall now prove that, if the earthquake be regular *without any discontinuity whatever* (which,

* The results arrived at in the previous sections may be easily shown experimentally by using weak and strong springs, so as to give M a naturally long or short period when it is set in vibration by shaking the frame to which it is attached. The motion can be magnified and indicated by a long light pointer moving over a scale rigidly attached to the frame, and the effects of introducing various amounts of friction shown by causing the pointer to rub with more or less force against the scale.

however, our experience of earthquakes in Japan leads us to believe is rarely the case), then weak springs will give good results.

Section D.

Let the earthquake motion be a periodic function of the time; then we know it may be expressed in the form

$$z = A_0 + \Sigma A \cos (Nt + F),$$

where A , N , and F may have any values we please in the successive terms of the series. Or, generally, it may be expressed in the form

$$z = \Sigma A \cos (Nt + F),$$

one N having a value nought.

Let us take the restricted case

$$z = \Sigma A \cos Nt;$$

then, if there is no discontinuity at the beginning,

$$z = 0,$$

and

$$\frac{dz}{dt} = 0$$

when

$$t = 0;$$

from which it may easily be shown that

$$\Sigma = 0.$$

The differential equation of the motion of the centre of the mass M relative to the box is

$$\frac{d^2x}{dt^2} + 2f \frac{dx}{dt} + n^2x = n^2 \Sigma E \cos Nt,$$

where

$$E = \frac{AN^2}{n^2}$$

for each value of A and N taken in the successive terms of the series. The solution of this differential equation is, if f is less than n ,

$$x = e^{-ft} C \cos (\sqrt{n^2 - f^2}t + D) - n^2 \Sigma \frac{E \cos \left(Nt + \tan^{-1} \frac{2Nf}{N^2 - n^2} \right)}{\sqrt{(N^2 - n^2)^2 + 4N^2 f^2}}, \quad (3)$$

the constants C and D being determined from the initial conditions, which are, when

$$t = 0,$$

$$x = 0,$$

and

$$\frac{dx}{dt} = 0;$$

from which it follows that

$$C \cos D = n^2 \Sigma J,$$

where

$$J = \frac{E(N^2 - n^2)}{(N^2 - n^2)^2 + 4N^2 f^2},$$

and

$$C^2 = n^4 \Sigma^2 J + \frac{n^4 f^2 (\Sigma J - \Sigma L)^2}{n^2 - f^2},$$

where

$$L = \frac{2EN^2}{(N^2 - n^2)^2 + 4N^2 f^2}.$$

It is obvious that we want the coefficients in the above equation for x to be proportional to nought, A_1, A_2, A_3 , &c., and also the epochs $\tan^{-1} \frac{2N_1 f}{N_1^2 - n^2}, \tan^{-1} \frac{2N_2 f}{N_2^2 - n^2}$, &c. to be all nought if we are to have a perfect representation of the earthquake. Now for the epochs to be very small, f being a reasonable coefficient of friction (say f equals $\frac{1}{2}n$), we must have $\frac{n}{N}$ either very small or very large. Examining the coefficients of the second part in equation (3),

$$\frac{n^2 E_1}{\sqrt{(N_1^2 - n^2)^2 + 4N_1^2 f^2}},$$

$$\frac{n^2 E_2}{\sqrt{(N_2^2 - n^2)^2 + 4N_2^2 f^2}}, \text{ \&c.,}$$

we see that the condition $\frac{n}{N}$ being very small will make them proportional to A_1, A_2 , &c., as is required for a perfect representation of the earthquake motion; and if we put the coefficient C into the form

$$C^2 = \Sigma^2 A \frac{1 - \frac{n^2}{N^2}}{\left(1 - \frac{n^2}{N^2}\right)^2 + 4 \frac{f^2}{N^2}}$$

$$+ \frac{1}{f^2 - 1} \Sigma^2 A \frac{1 + \frac{n^2}{N^2}}{\left(1 - \frac{n^2}{N^2}\right)^2 + 4 \frac{f^2}{N^2}},$$

we observe that when $\frac{n}{N}$ is *very* small and f the same as above,

$$C = \frac{2}{\sqrt{3}} \Sigma A.$$

Now, as ΣA being equal to nought, as previously shown, is the condition of continuity, C disappears; and hence all earthquakes which have continuity from the beginning, and which are expressible in the form

$$z = \Sigma A \cos Nt,$$

are perfectly represented if n is very small compared with every N —that is, if the natural vibration of the spring has a period much longer than the period of any element of the earthquake. This also introduces the additional restriction that no N can be very small; consequently z cannot have a constant term. If in the above f is nearly equal to n , then

$$C = \text{a very large number} \times \Sigma A.$$

If ΣA is absolutely nought, then the size of the multiplier is of no consequence; but if ΣA is not absolutely nought (that is, if there is a slight discontinuity), then C may be very large; so that with more friction the failure of the weak springs to produce an accurate registering apparatus is very much more marked. And since the coefficient e^{-ft} is greater than e^{-nt} and e^{-nt} is large since n is small, it follows that e^{-ft} will be large, and will not rapidly reduce the value of the first term (which may be said to belong to the natural vibrations of the springs) in equation (3) for x .

We shall now consider the alternate condition, viz.

$$\frac{n}{N} \text{ very large;}$$

i. e.

$$\frac{N}{n} \text{ very small, or the springs very strong.}$$

The coefficients in the second part of equation (3) may be put in the form

$$\frac{A_1 \frac{N_1^2}{n^2}}{\sqrt{\left(\frac{N_1^2}{n^2} - 1\right)^2 + \frac{4f'^2 N_1^2}{n^4}}},$$

$$A_2 \frac{N_2^2}{n^2}$$

$$\frac{\sqrt{\left(\frac{N_2^2}{n^2} - 1\right)^2 + \frac{4f'^2 N_2^2}{n^4}}},$$

&c.;

and as $\frac{f}{n^2}$ is assumed to be equal to $\frac{1}{4}$, we see that the denominator may be regarded as constant and very little less than unity; consequently the second, third, &c. terms of equation (3) will be proportional to $A_1 N_1^2$, $A_2 N_2^2$, &c. It follows therefore that the elementary vibrations of the earthquake, of smaller periodic times than the rest, will, in the representation, have greater amplitudes than they ought to have. If, however, the elementary periods are not very unequal, the curve drawn by the seismograph will be a fairly approximate representation of the earthquake.

C may be expressed in the form

$$C^2 = \Sigma^2 A \frac{N^2}{n^2} \frac{\frac{N^2}{n^2}}{\left(\frac{N^2}{n^2} - 1\right)^2 + 4 \frac{f^2}{n^4} N^2} + \frac{1}{\frac{n^2}{f^2} - 1} \Sigma^2 A \frac{N^2}{n^2} \frac{\frac{N^2}{n^2} + 1}{\left(\frac{N^2}{n^2} - 1\right)^2 + 4 \frac{f^2}{n^2} N^2},$$

or, since $\frac{f^2}{n^2}$ equals $\frac{1}{4}$, and $\frac{N}{n}$ is very small,

$$C = \frac{f}{\sqrt{n^2 - f^2}} \Sigma \frac{A N^2}{n^2};$$

therefore C is a very little less than the algebraical sum of the other coefficients; and if we suppose, as above, that the values of N_1 , N_2 , &c. are not very different, then, since ΣA equals nought, it follows that $\Sigma \frac{A N^2}{n^2}$, and therefore C, cannot be very great.

We therefore conclude that, in the case of an earthquake represented by the equation

$$z = \Sigma A \cos Nt,$$

both weak and strong springs give good results.

As an example, let the earthquake motion be represented by

$$z = A (\cos kt - \cos \frac{11}{9} kt),$$

the curve corresponding with which is shown by the thick white line $\gamma\gamma\gamma$ in fig. 2, the thin line $\alpha\alpha\alpha$ being

$$x = A \cos kt,$$

and the thin line $\beta\beta\beta$

$$x = -A \cos \frac{11}{9} kt.$$

Let the springs be strong, so that

$$n = \frac{100}{9} k,$$

and let

$$f = \frac{n}{2};$$

then, determining the values of the constants, we find

$$\begin{aligned} x = & -e^{-5.5kt} \times 0.00458 A \cos(9.62kt + 0.5142) \\ & - 0.00814 A \cos(kt - 0.0917) \\ & + 0.01218 A \cos\left(\frac{11}{9}kt - 0.1096\right), \end{aligned}$$

the curve corresponding with which is shown by the thick white line *iii* in fig. 3, the thin line $\delta\delta\delta$ being

$$x = -e^{-5.5kt} \times 0.00458 A \cos(9.62kt + 0.5142),$$

the thin line $\eta\eta\eta$

$$x = -0.00814 A \cos(kt - 0.0917),$$

and the thin line $\theta\theta\theta$

$$x = 0.01218 A \cos\left(\frac{11}{9}kt - 0.1096\right).$$

This curve (*iii*) is magnified so that the greatest amplitude is nearly the same as the greatest amplitude in the real earthquake motion.

If the springs remain as before, but if there be no friction, then, determining the constants, we find

$$\begin{aligned} x = & -0.00408 A \cos(11.1kt) \\ & - 0.00816 A \cos(kt) \\ & + 0.01224 A \cos(1.2kt). \end{aligned}$$

If the earthquake be represented by the same equation as before, and if

$$n = 3k$$

and

$$f = \frac{1}{2}n,$$

then equation (3) becomes

$$\begin{aligned} x = & -e^{-\frac{3}{2}kt} \times 0.071831 A \cos(2.55kt + 0.78015) \\ & - 0.11704 A \cos(kt - 0.35896) \\ & + 0.17877 A \cos\left(\frac{11}{9}kt - 0.45408\right); \end{aligned}$$

the curve corresponding with which is shown by the thick white line $\nu\nu\nu$ (fig. 4), the thin line $\kappa\kappa\kappa$ being

$$x = -e^{-\frac{3}{2}kt} \times 0.071831 A \cos(2.55kt + 0.78015),$$

the thin line $\lambda\lambda\lambda$

$$x = -0.11704 A \cos(kt - 0.35896),$$

and the thin line $\mu\mu\mu$

$$x = 0.17877 A \cos\left(\frac{11}{9} kt - 0.45408\right).$$

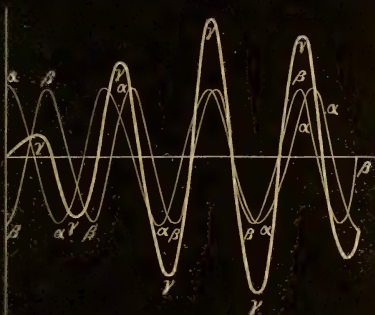


Fig. 2.



Fig. 3.

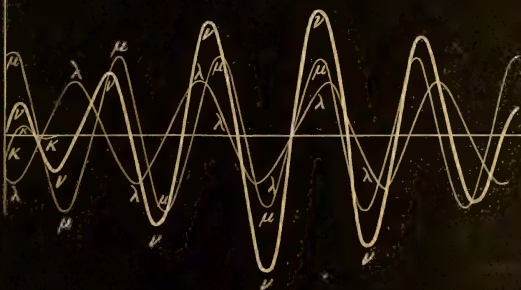


Fig. 4.

Section E.

Let us now consider the case when

$$f=0;$$

then, unless n equals N , all the epochs are nought; so that, as far as the epochs are concerned, no restriction is introduced in making the seismograph-curve exactly represent the earthquake. C may be put into the form

$$\Sigma \frac{A}{1 - \frac{n^2}{N^2}};$$

so that if n is small compared with every N (that is, if the springs are weak), then

$$C = \Sigma A \text{ nearly,} \\ = 0 \text{ nearly,}$$

since $\Sigma A = 0$ is the condition of continuity; and the coefficients of the second part of the right-hand side of equation (3) are

$$\frac{A_1 N_1^2}{N_1^2 - n^2}, \quad \frac{A_2 N_2^2}{N_2^2 - n^2}, \quad \&c.,$$

which are equal to $A_1, A_2, \&c.$ Therefore weak springs give a very good representation of the earthquake.

It must, however, be remembered that, although in the case of an earthquake motion having no discontinuity, weak springs give good results, sometimes even better than strong springs, still in the most complicated cases, if the natural vibrations of the springs be quick, a little experience will enable easy corrections to be made, which will allow the real earthquake motion to be read with much greater accuracy from the representations than might at first sight appear from the formulæ.

Section F.

Without the help of actual experiments made with the form of seismograph we propose, calculation leads us to infer that with a small amount of friction, such as that opposed to a lead ball, of say 400 lbs. mass, surrounded by water or oil, and with the ball moving a simple recording apparatus, strong springs will always give much more satisfactory results than weak ones for earthquakes such as we have felt in Japan; but since friction will always cause the natural vibrations of the ball to cease in a longer or shorter time according as the friction is small or great, it is possible that very satisfactory

results may be obtained by using weak springs and surrounding the ball with some more viscous liquid, such as treacle or a mixture of tar and pitch, if only the alteration of viscosity by change of temperature can be readily compensated for, and if it be possible to easily employ a good recording apparatus when using much friction. On the whole our calculations point to the employment of a small amount of friction and strong springs; but for the following reasons we feel that our calculations should be chiefly employed only for directing and making use of experimental knowledge:—first, because we assumed the friction to be proportional to the mass of M and to its velocity, whereas in reality the friction is probably composed partly of a constant term depending on the recording apparatus, and partly of a term proportional to the square of the velocity of M , and to a certain extent proportional to its surface; secondly, because extremely little is at present known regarding the nature of the earthquake shock. Even the period of an earthquake vibration does not seem to have been measured with any approach to accuracy; the information obtained in some cases from the stopping of clock-pendulums is quite unsatisfactory, since the limits between which we can place the period of earthquake vibrations so as to stop an ordinary pendulum-clock are wide apart.

Section G.

Fig. 5 (Pl. IV.) shows roughly our first idea of the construction of a seismometer in accordance with the principles we have enunciated. A leaden ball of some 400 lbs. mass is supported by five strong spiral springs inside a strong iron case, rigidly fixed to the rocky crust of the earth; four of the springs are horizontal, and one vertical, and all have the same period; so that if there were no friction the centre of mass of M would describe an ellipse when M is freely vibrating. In order to get a record of north-south, east-west, and up-down motion of M , three arms, two of which (AB , CD) are shown in the figure, carry pencils pressed by means of small spiral springs on a band of paper moved regularly by clockwork in a horizontal direction at right angles to BD —the clockwork, as in Professor Palmieri's and other instruments, being set in motion at the commencement of the earthquake. The arm AB is rigidly fixed to a small piece AE at right angles to it (see figure of ball enlarged); this again by means of a pivot at E is fixed to EF , which is rigidly attached to the ball. A pin at A , supported by the framework of the instrument, allows AB to move round it, and so to record vertical motions of the ball; and the pin A , having a certain amount of lateral motion in the slot, combined with the

shape of A E F, prevents A B recording any lateral motions, since the motion of A B parallel to itself is so small as to be imperceptible. C D turns about the pin C, and is prolonged to G, where it is attached by a pivot to an eye rigidly attached to the ball: G C D therefore records lateral motions in one direction, say north-south; but is not effected by east-west motions or by vertical motions of the ball, as these latter only cause the pin C to move vertically in the slot. The third arm, not shown in the figure, by a somewhat similar arrangement of levers, only records east-west motions. All the motions are recorded on one plane, on the same band of paper; so that the curves would be somewhat as shown in fig. 6. Drawing any line A B at right angles to the motion of the paper, we see that at that moment of time the ball was moving from south to north, from west to east, and from up to down; and from the shape of the curves we can determine the position, velocity, and acceleration in magnitude and direction of the ball at that or any other instant of time: the complete law of the motion of the ball is therefore recorded. Should the box be slightly tilted and some of the springs elongated or shortened during the disturbance, then the motions will not be strictly north-south, east-west, &c.; but it is evident that this cannot produce any serious discrepancy in the indications unless the earthquake motions be exceedingly violent, and when this is the case it will not be very difficult to eliminate the errors.

It is evident the points B and D may be above the ball instead of below it as in the figure; and this arrangement would be preferable when we wish to surround the ball with a liquid, as the paper could then be kept quite clear of the liquid.

Section H.

Mr. Mallet is of opinion that there is no turning action of the ground during earthquakes; but it is possible this conclusion may be perhaps a little premature, since any explanation that has been given of the observed twisting of columns, based on considerations of the attachment at the base, might also apply to the twisting of rock in its natural position. To test whether any such turning action really exists, a simple apparatus, such as is shown in fig. 7, might be employed. H J is an iron fly-wheel rigidly attached by a stretched thick wire K L to a rigid iron framework K M N, or by a thinner wire if lateral motion of the flywheel is prevented by a guide. An arm J P carries a pencil at P touching a band of paper (the one, for example, employed in the previous seismometer) moved by clockwork parallel to M N. If the periodic time of the torsional vibration

of H J be, say, one fifth of that of the earthquake, then any rotatory vibration of the earth will be well recorded*.

The observations of Mr. Mallet, made at the scene of the Neapolitan earthquake of 1857, are of great value in connexion with the science of seismometry, which owes its growth in a great measure to the labours of that gentleman. But we have no hesitation in saying that *three* recording seismographs, such as we have described, suitably placed in the plain of Yedo and with clocks in telegraphic communication with one another, would give more information regarding earthquakes in a few months than could be obtained by the most experienced observers from the remains of many destroyed cities. We are aware of the great interest now being taken by the German Asiatic Society in the subject of seismometry; and it is to a certain extent in consequence of this that we have been led to publish this paper. A not very extended series of experiments would probably be all that would be required before we could furnish working drawings of an almost perfect recording instrument; and after such instruments had been constructed the Japanese Government might possibly be induced to allow them to be used at their telegraph offices. With very little extra expense these seismometric records might be supplemented by regular observations of the natural currents in the telegraph-lines, to the importance of observing which we have recently directed attention in a former paper†.

ADDITION, May 1879.

It is interesting to notice that the principles developed mathematically in this paper have, since it was written, been arrived at by natural selection in the relatively *rigid naturally*

* Since writing this paper a rather sharp earthquake has been experienced in Tokio, which caused the scale-pans of a balance in the Physical Laboratory of the Imperial College of Engineers to describe perfect circles, the chains (about 35 centimetres long) which supported the pans and the pans themselves moving like a conical pendulum. The radius of the circle described by each pan at the beginning was about 5 centimetres; and the motion continued for a long time after the earthquake had ceased. The circular motion was probably produced by two successive shocks being nearly at right angles to one another.

Some time after this earthquake we constructed an exceedingly sensitive rotatory seismograph on the general principle shown in fig. 7, the wire K L being about ten feet long; but up to the date of one of the authors leaving Japan last year the instrument gave no evidence of any earthquake producing a turning action by a single shock.

† "The Importance of a General System of Simultaneous Observations of Atmospheric Electricity," Transactions of the Asiatic Society of Japan, vol. v. part 1, p. 131, and Journal of the Society of Telegraph Engineers, vol. vi. p. 242.

rapid vibratory diaphragm of the telephone and phonograph of Professor Bell and Mr. Edison. For just as the receiving diaphragm, from the non-preponderating character of its *very rapid* natural vibrations, is able to produce *truthfully in miniature* the varied sound-vibrations communicated to the transmitting diaphragm, so does our seismograph *truthfully record in miniature* the earthquake shocks it has received.

And we are led to think that, if rapidity of vibration be combined with viscous resistance (as explained in the paper) telephones and phonographs may be successfully constructed of far larger dimensions and of far greater power than has hitherto been attempted.

V. *Induction-balance and Experimental Researches therewith.*
By Prof. D. E. HUGHES*.

[Plate V.]

IMMEDIATELY upon the announcement of Arago's discovery of the influence of rotating plates of metal upon a magnetic needle (1824), and Faraday's important discovery of voltaic and magneto-induction (1831), it became evident that the induced currents circulating in a metallic mass might be so acted upon either by voltaic or induced currents circulating in a metallic mass as to bring some new light to bear on the molecular construction of metallic bodies.

The question was particularly studied by Babbage, Sir John Herschell, and M. Dove, who constructed an induction-balance, wherein two separate induction-coils, each having its primary and secondary coils, were joined together in such a manner that the induced current in one coil was made to neutralize the induced current in the opposite coil, thus forming an induction-balance, to which he gave the name of "differential inductors." In those days physicists did not possess the exquisitely sensitive galvanometers and other means of research that we possess today; but sufficiently important results were obtained to prove that a vast field of research would be opened if a perfect induction-balance could be found, together with a means of correctly estimating the results obtained. In experimenting with the microphone I had ample occasion to appreciate the exquisite sensitiveness of the telephone to minute induced currents. This led me to study the question of induction by aid of the telephone and microphone: the results of those researches have already been published. Continuing this line of inquiry, I thought I might again attempt to investigate the molecular construction of metals and alloys; and with this object I have obtained, after numerous compara-

* Communicated by the Physical Society.

tive failures, a perfect induction-balance which is not only exquisitely sensitive and exact, but allows us to obtain direct comparative measures of the force or disturbances produced by the introduction of any metal or conductor.

The instrument now submitted to the Physical Society consists, 1st, of the new induction-currents balance; 2nd, a microphone, with a clock as a source of sound; 3rd, electric sonometer, or absolute sound-measurer, a late invention of my own; 4th, a receiving telephone and three elements of Daniell's battery. In order to have a perfect induction-currents balance suitable for physical research, all its coils as well as the size and amount of wire should be equal. The primary coils a, a' , and the secondary coils b, b' , should be separated and not superposed. The exterior diameter of the coils is $5\frac{1}{2}$ centims., having an interior vacant circular space of 3 centims.; the depth of this flat coil or spool is 10 millims. Upon this box-wood spool are wound 100 metres of No. 32 silk-covered copper wire. I use four of such coils, formed into two pairs, the secondary coil being fixed permanently, or by means of an adjustable slide, at a distance of 5 millims. from its primary; on the second similar pair there is a fine micrometer-screw, allowing me to adjust the balance to the degree of perfection required. These two pairs of coils should be placed at a distance not less than one metre from each other, so that no disturbing cause may exist from their proximity. The two primary coils are joined in series to the battery, the circuit also passing through the microphone. In place of the telephone I have sometimes used a magnetic pendulum, the swing or the arc described indicating and measuring the forces. I am at present engaged upon a very sensitive voltmeter, which shall indicate and measure the force of rigid induced currents. The telephone, however, is well adapted as an indicator, but not as a measure of the forces brought into action. For this reason I have joined to this instrument an instrument to which I have given the name of electric sonometer (Pl. V.) This consists of three coils c, d, e , similar to those already described, two of which are placed horizontally at a fixed distance of about 40 centims. apart; and the communication with battery is so arranged that there are similar but opposing poles in each coil; between these two there is a coil, d , which can be moved on a marked sliding scale, f , divided into millimetres, in a line with these two opposing primary coils*. This central coil is the secondary one, and connected by means of a switching key with the telephone in place of the induc-

* If the coils c and e are of unequal size, the zero of the scale will occupy a position similar to that shown on the Plate.

tion-balances. If this secondary coil is near either primary coil, we hear loud tones, due to its proximity. The same effect takes place if the secondary coil is near the opposing coil, except that the induced current is now in a contrary direction, as a similar pole of the primary acts now on the opposite side of the induction-coil. The consequence is, that as we withdraw it from one coil and approach the other, we must pass a line of absolute zero, where no current whatever can be induced, owing to the absolute equal forces acting equally on both sides of the induction-coil. This point is in the exact centre between the two coils.

We thus possess a sonometer having an absolute zero of sound: each degree that it is moved is accompanied by its relative degrees of increase; and this measure may be expressed in the degrees of the millimetres passed through, or by the square of the distances in accordance with the curve of electro-magnetic action. If we place in the coils of the induction-balance a piece of metal (say copper, bismuth, or iron), we at once produce a disturbance of the balance, and it will give out sounds more or less intense on the telephone according to the mass, or if of similar sizes, according to the molecular structure of the metal. The volume and intensity of sound is invariably the same for a similar metal. If by means of the switching key the telephone is instantly transferred to the sonometer, and if its coil be at zero, we hear sounds when the key is up, or in connexion with the wire *g*, which leads to the induction-balance, and no sounds, or silence, when the key is down or in contact with the wire *h*, in connexion with the sonometer. If the sonometer-coil were moved through several degrees, or through more than the required amount, we should find that the sounds increase when the key is depressed; but when the coil is moved to a degree where there is absolute equality, if key is up or down, then the degree on scale should give the true value of the disturbance produced in the induction-balance; and this is so exact that if we put, say, a silver coin whose value is 115° , no other degree will produce equality. Once knowing, therefore, the value of any metal or alloy, it is not necessary to know in advance what the metal is; for if its equality is 115° , it is silver coin; if 52 , iron; if 40 , lead; if 10 , bismuth; and as there is a very wide limit between each metal, the reading of the value of each is very rapid, a few seconds sufficing to give the exact sound-value of any metal or alloy.

The respective values of the different metals may, as I have already pointed out, be indicated by introducing the sonometer into circuit. I find, however, that it is difficult to estimate fractional differences when the sounds to be com-

pared are loud. I therefore prefer to *balance* the metal under examination by means of a similar mass placed in the opposing coil, reading on the sonometer the differences of sound, which are then slight. Experience has shown that the most accurate results are obtained when the sonometer is replaced by a graduated strip of zinc about 23 millims. wide, 200 millims. long, and tapering from a thickness of 4 millims. at one end to a fine edge at the other, and superposed in a horizontal plane over the opposing coil b' , the metal to be tested being in a plane midway between a and b on the left of the plate.

The delicacy of the readings may of course be greatly increased by diminishing the angle between the two faces of the strip; but there are many points connected with its use which would be too long to describe in this paper.

As a rule three Daniell elements will be found quite sufficient; and even this weak force is so exquisitely sensitive that it will find out the smallest fraction of difference in weight or structure of metals. Thus two silver coins such as a shilling, both quite new, and both apparently of the same weight, will be found to possess a difference of weight which the instrument at once indicates.

The following experiments will show its exceeding sensitiveness and its wide field of usefulness as an instrument of research.

I. If we introduce into one pair of the induction-coils any conducting body, such as silver, copper, iron, &c., there are set up in these bodies electric currents which react both upon the primary and secondary coils, producing extra currents whose force will be proportional to the mass and to its specific conducting-power. A milligramme of copper or a fine iron wire, finer than the human hair, can be loudly heard and appreciated by direct measurement, and its exact value ascertained. We can thus weigh to an almost infinitesimal degree the mass of the metal under examination: for instance, if we take two English shilling pieces fresh from the Mint, and if they are absolutely identical in form, weight, and material, they will be completely balanced by placing one each in the two separate coils, provided that for these experiments there is an adjustable resting-place in each pair of coils, so that each coin may lie exactly in the centre of the vacant space between the primary and secondary coils. If, however, these shillings are in the slightest degree worn, or have a different temperature, we at once perceive this difference, and, if desired, measure it by the sonometer, or by lifting the supposed heaviest coin at a slight distance from the fixed centre line: the amount of degrees that the heaviest coin is withdrawn will show its relative mass or weight as compared with the lightest. I have thus been able to appreciate the difference

caused by simply rubbing the shilling between the fingers, or the difference of temperature by simply breathing near the coils; and in order to reduce this sensibility within reasonable limits, I have only used in the following experiments 100 metres of copper wire to each coil, and 3 cells of battery.

II. The comparative disturbing value of disks of different metals, all of the same size and form of an English shilling, and measured in millimetre-degrees, by the sonometer, is the following:—

Silver (chemically pure)	125
Gold	117
Silver (coin)	115
Aluminium	112
Copper	100
Zinc	80
Bronze	76
Tin	74
Iron (ordinary).....	52
German silver	50
Iron (chemically pure).....	45
Copper (antimony alloy)	40
Lead	38
Antimony	35
Mercury	30
Sulphur (iron alloy).....	20
Bismuth.....	10
Zinc (antimony alloy)	6
Spongy gold (pure).....	3
Carbon (gas).....	2

III. It will be seen from the above that the instrument gives very different values for different metals or alloys; consequently we cannot obtain a balance by employing two disks of different metals, and the instrument is so sensitive to any variation in mass or matter that it instantly detects the difference by clear loud tones on the telephone. If I place two gold sovereigns of equal weight and value, one in each coil, there is complete silence, indicating identity or equality between them; but if one of them is a false sovereign, or even gold of a different alloy, the fact is instantly detected by the electrical balance being disturbed. The instrument thus becomes a rapid and perfect coin-detector, and can test any alloy, giving instantly its electrical value. The exceeding sensitiveness of this electrical test I shall demonstrate by experiment, now. Again, as regards coins, it resolves an almost magical problem. Thus, if a person puts one or several coins into one pair of coils, the amount or nominal value being un-

known to myself, I have only to introduce into the opposite coils different coins successively, as I should weights in a scale, and when perfect balance is announced by the silence, the amount in one box will not only be the same nominal value but of the same kind of coin.

IV. We find, by direct experiment with this instrument, that the preceding results are due to electric currents induced by the primary coil, and that it is by the reaction of these that the balance is destroyed; for if we take an insulated spiral disk or helix of copper wire with its terminal wires open, there is no disturbance of the balance whatever, notwithstanding that we have introduced a comparatively large amount of copper wire; but on closing the circuit the balance is at once very powerfully disturbed.

If the spiral is a flat one, resembling a disk of metal, and circuit closed, we find that loud tones result when the spiral is placed flat, or when its wire is parallel to those on the coils; but if it is held at right angles to these wires, no sound whatever is heard, and the balance remains perfect. The same thing occurs with disks of all non-magnetic metals, and a disk of metal placed perpendicular to the coils exerts no influence whatever. The contrary result takes place with a spiral of iron wire or disk of iron: the induced current circulating in the spiral is at its maximum when the spiral lies flat or parallel with the coils, giving no induced current whatever when at right angles; but the disturbances of the induction-balance are more than fourfold when perpendicular to the wires of the coils than when parallel with the same. This result is simply due to the property of magnetic bodies, of conduction of magnetism.

V. If we introduce a disk of metal gradually into the coils, we find that its power increases as the square of the distance, until it arrives at its maximum exactly in the centre of the vacant space between each pair of coils, diminishing rapidly in the same ratio if the disk be moved towards primary or secondary. Thus, in the interior of the coils there is but a single line of maximum force; but at the exterior we have on each coil two maxima and one minimum, the first and most powerful maximum being in the centre of vacant space between each pair of coils, coinciding with the maximum lines of force of the centre of the coils; the minimum lines of force are exactly in the exterior centre of each coil, again rapidly rising to a maximum near the exterior edge of coils, and gradually diminishing in power from this point.

If we place exteriorly a bar of metal in the centre line of vacant space, we find that it has here its maximum disturbing power, giving out loud sounds.

If we now move this bar until it rests on the centre of either coil, we find that at this point the bar has no disturbing effect whatever, and although the coils at its maximum line of force are sensitive to the finest iron wire, a very large mass of iron or a rod of 1 centimetre diameter has no disturbing effect whatever. Its paralyzing effects are so remarkable, that if we place a flat piece of iron or other metal across the maximum line of force, where loud sounds are given out, the instant that this flat piece is moved, so that one or both edges touch or end in the minimum line of force, it becomes instantly neutralized, giving out no sound whatever, notwithstanding that a large mass of metal lies in the maximum line of force. We will now demonstrate by experiment the exceeding sensitiveness of the induction-balance to the smallest piece of metal, if it is in the maximum line of force, and no part of it touching the minimum; and also that by allowing either or both ends of this or a very large piece of metal to cut or end at the minimum lines of force, complete paralysis and consequent silence are produced*.

VI. There is a marked difference of the rapidity of action between all metals, silver having an intense rapidity of action. The induced currents from hard steel or from iron strongly magnetized are much more rapid than those from pure soft iron; the tones are at once recognized, the iron giving out a dull, heavy smothered tone, whilst hard steel has tones exceedingly sharp. If we desire to balance iron, we can only balance it by a solid mass equal to the iron to be balanced. No amount of fine wires of iron can balance this mass, as the time of discharge of these wires is much quicker than that of a larger mass of iron. Hard steel, however, can be easily balanced not only by steel but by fine iron wires, and the degree of the fineness of these wires required to produce a balance gives a very fair estimate of the proportionate time of discharge. The rapidity of discharge has no direct relation with its electrical conductivity; for copper is much slower than zinc, and they are both superior to iron.

We find that the induction-balance is exceedingly sensitive to all molecular changes which take place in all metals subjected to any of the imponderable forces. Thus we have already by its aid studied the effects on metals of heat, magnetism, electricity, &c., and of mechanical changes such as strain, torsion, and pressure; and I propose in some future paper to describe the remarkable results already obtained, and to demonstrate by experiment today to the Physical Society the results of these forces upon the induction-balance.

* This was fully demonstrated to the audience by numerous experiments.

VI. *Note on the Examination of certain Alloys by the Aid of the Induction-balance.* By W. CHANDLER ROBERTS, F.R.S., *Chemist of the Mint**.

[Plate VI.]

SOME weeks since, Prof. Hughes showed me that equal volumes of various metals give widely different indications with the induction-balance. It appeared probable that a careful examination of a definite series of alloys would prove to be of interest; and as Prof. Hughes at once gave me the most generous assistance, teaching me the manipulation and controlling the results, I am able to submit the following observations to the Society.

The relative values of different metals as indicated by the induction-balance were given by Prof. Hughes in a paper read before the Royal Society on the 15th of May last. They do not accord with the values usually accepted as representing the relative conductivity of the respective metals; and this being the case, it became important to ascertain what relation the indications given by alloys, when under the influence of the induced current, bear to their electric conductivities, which afforded Matthiessen a basis for dividing them into groups†.

A series typical of each group was therefore taken; the constituent metals were melted together in the requisite proportions; and the thoroughly mixed alloys were carefully rolled to a uniform thickness, usually 1·3 millim. Disks 24 millims. in diameter were then cut with the same punch; and these disks were placed in succession on one side of the balance, so that their bases lay exactly on a line midway between the primary and secondary coils, this having been found to be the plane of maximum force. The respective values of the alloys were ascertained either by introducing the sonometer into circuit, or by superposing a graduated wedge-shaped scale of zinc over the opposing coil of the balance, as has already been explained by Prof. Hughes.

The alloys of Lead and Tin were selected as an example of Matthiessen's first group. The results are recorded in the following Table, and are graphically indicated in the curve No. 1, Plate VI. The readings are those of the zinc scale‡.

The Gold-Silver alloys, representing the second group, presented no difficulty of manipulation; and the observations were made on disks 1·3 millim. thick and 24 millims. in dia-

* Communicated by the Physical Society.

† British-Association Report, 1863, p. 37.

‡ The use of a scale of greater accuracy than the one employed may slightly alter some of the figures, but it can hardly change the general nature of the curves.

meter. The results are given in the Table and in curve No. II.

The alloys of Tin and Copper, taken as representative of the third group, are peculiar. Their tints and fractures are widely different; and the series is interesting as having various industrial applications. As many of them are too brittle to roll, a block of each alloy 18 millims. square by 7 millims. thick was formed with the file. The results are given in the Table and on the curve No. III.

No.	Percentage composition.	Approximate formula.	Readings on Induction-balance.
TIN-LEAD.	1. 100 (pure tin).		67.5
	2. 77.37	$\text{Sn}_6 \text{Pb}$	62.0
	3. 69.60	$\text{Sn}_4 \text{Pb}$	59.0
	4. 53.20	$\text{Sn}_2 \text{Pb}$	52.5
	5. 36.30	Sn Pb	51.0
	6. 22.30	Sn Pb_2	47.0
	7. 12.50	Sn Pb_4	45.0
	8. 8.60	Sn Pb_6	46.0
	9. 0 (pure lead).		43.0
SILVER-GOLD.	1. 100 (pure silver).		225
	2. 99.97		209
	3. 99.90		205
	4. 99.50		192
	5. 99.10		170
	6. 98.02		148
	7. 94.93		115
	8. 90.00		84
	9. 81.40	$\text{Ag}_3 \text{Au}$	60
	10. 68.70	$\text{Ag}_4 \text{Au}$	48
	11. 52.30	$\text{Ag}_2 \text{Au}$	44.5
	12. 35.50	Ag Au	42.8
	13. 21.50	Ag Au_2	44
	14. 12.00	Ag Au_4	49
	15. 8.30	Ag Au_6	60
	16. 4.1		90
	17. 0 (pure gold).		150
COPPER-TIN.	1. 100 (pure copper).		167
	2. 89.00	Sn Cu_{15}	65
	3. 84.33	Sn Cu_{10}	51
	4. 79.02	Sn Cu_7	45
	5. 72.91	Sn Cu_5	40
	6. 68.28	Sn Cu_4	37
	7. 65.00		49
	8. 61.79	Sn Cu_3	83
	9. 51.84	Sn Cu_2	73
	10. 34.99	Sn Cu	73
	11. 9.73	$\text{Sn}_5 \text{Cu}$	82
	12. 0 (pure tin).		85

Note.—The Alloys were not annealed; and the temperature was about 15° C.

If the curves for Lead-Tin and Gold-Silver are compared with those given by Matthiessen* for the same alloys, their similarity will at once be evident. On the other hand, the induction-balance curve of the Tin-Copper series, while bearing some general resemblance to Matthiessen's curve of conductivity, differs essentially from it in certain parts. Matthiessen's curve falls rapidly from 93 (the conductivity of pure copper) to 9 (that of the alloy containing 85 volumes per cent. of copper). It then passes horizontally in a line which is approximately straight to 13, the conductivity of Tin.

Some light would appear to be thrown on the difference between the two curves by the work of M. Alfred Rich† on the density of alloys of copper and tin. He showed that copper and tin contract in alloying, the contraction being regular from pure tin up to the alloy containing 38 per cent. of tin, the density of which is higher than that of pure copper. M. Rich's experiments were conducted on alloys both in the form of powder and ingots; the latter have alone been given in the curve marked with his name on the Plate; and the relation between the two curves, especially at the points a , a' , b , b' , is too evident to need comment.

It may ultimately prove that if the alloys were rolled or compressed the curve would be modified; and, on the other hand, further experiments on the conductivity of the alloys may reveal points of identity between the conductivity and induction-balance curves; the part where the former from being vertical becomes horizontal would be especially worth examination. It may be well to point out that the alloys SnCu_3 and SnCu_4 , which occupy critical positions on the induction curve, have been shown by M. Rich to be singularly free from the disturbing influence of liquation.

The work would appear to be interesting as showing that the induction-balance may afford a simple means for detecting variations in the molecular structure of alloys and for indicating allotropy in metals with greater accuracy than has hitherto been possible.

Practical application.—The possibility of ascertaining the standard fineness of alloys by the aid of electricity long ago occupied the attention of physicists. In 1823 M. Becquerel‡ suggested that trustworthy indications might be afforded by the electromotive force developed when the alloy is placed in an exciting fluid, together with an alloy of known composition.

* *Op. cit.* p. 46, and Watts's Dictionary of Chemistry, vol. iii. p. 943.

† *Ann. de Chimie et de Phys.* tome xxx. 1873.

‡ *Ibid.* t. xxiv. p. 343 (1823).

The subject was partially investigated by Ørsted in 1828*; and as its practical importance was further indicated by Gay-Lussac in 1830†, I made a series of experiments in order to ascertain how far the more delicate appliances in use at the present day could be made available. The results, however, were not entirely satisfactory.

Prof. Hughes's Induction-balance rendered it possible to resume the research on a new basis. It is only necessary to glance at such a curve as that of the Gold-Silver series No. II. to be satisfied of the probability that certain parts of it, at least, would indicate minute differences of standard. I would therefore direct special attention to the series of alloys which lie between pure silver and silver alloyed with 5 per cent. of gold. These are shown in a separate curve, where the scale of percentages is more extended. Such alloys as Nos. 2 to 6 are known to refiners as doré; and No. 2 contains less than 2 grains of gold to the pound troy, a quantity which could not be extracted with profit by the ordinary operation of "parting." Small as the amount of precious metal is, its presence is clearly indicated on the induction curve, as are also the larger amounts of gold contained in Nos. 3-5.

Experiments are in progress on other series which promise to afford trustworthy indications; but of course the establishment of a method of verifying the composition of alloys of the precious metals must in part depend on the degree to which the presence of traces of foreign metals influence the accuracy of the results.

My object in these notes is not to insist on any particular application, but to bear testimony from a metallurgical point of view to the delicacy and simplicity of the instrument which Prof. Hughes has placed at our disposal; and I would offer him my sincere thanks for the liberal aid he has so readily given me.

VII. On the Theory of Faults in Cables.

By OLIVER HEAVISIDE.

1. **T**HE only kind of fault to be here considered is either a local defect in the insulation, or an artificial connexion between the conductor of a cable and the earth. When a fault occurs in a submarine cable, its most manifest effect on the working is to increase the strength of current leaving the sending end, because the resistance is reduced; while at the same time the strength of current arriving at the distant

* *Ann. de Chimie et de Phys.* t. xxxix. 1828, p. 274.

† *Instruction sur l'Essai des Matières d'Argent par la Voie Humide.* Paris, 1830.

end is reduced, the loss of current through the fault being greater than the increase in the current leaving the sending end. Another effect is to increase the speed at which signals can be made through the cable. A cable may be considered electrically as a long cylindrical condenser, or as a conductor having a great number of condensers of small capacity connected at equidistant points to it on the one side and to earth on the other. When an electromotive force is applied at one end, to establish a permanent current in the circuit these condensers have to be charged, an operation requiring time for its fulfilment; and before the current can cease when the electromotive force is removed, the charge must be got rid of: in fact, the current results from the discharge of the cable's electricity. If there is a fault, the discharge of the cable is facilitated; for there is not only a smaller quantity of electricity to be discharged, but more paths are open to it. Similarly the charging of the cable is facilitated, as will be seen by supposing the cable when uncharged to contain two exactly equal and opposite charges. Let one of these discharge itself. The cable will then become charged with the other; and since the discharge of the first is facilitated, the charging of the cable by the second is also facilitated. With a fault, a smaller quantity of electricity is required in order to produce the permanent state of electrification when an electromotive force is applied at one end of the line than when there is no fault; therefore, other things being the same, a given fraction of the final permanent state is more quickly reached in the former than in the latter case. Similarly the effect of a given signal is more rapidly dissipated in the former than in the latter case; and consequently from both these causes signals can be packed more closely together when the cable is faulty; or, in other words, the speed of working can be increased with equal legibility.

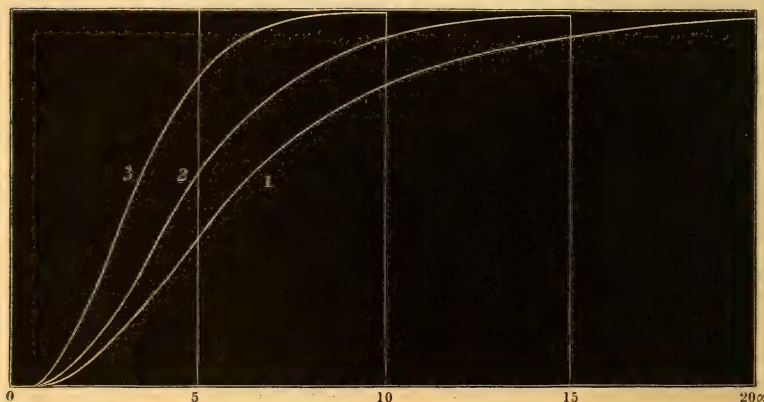
2. Before proceeding to the mathematics of the subject, I give some of the calculated arrival curves in simple cases. Referring to fig 1, suppose in the first place the cable is perfectly insulated and free from charge, and that both ends are to earth. At a given time $t=0$, introduce a constant electromotive force E at one end P of the cable. Then the well-known curve of arrival of the current at the distant end Q is represented by curve 1. Time is measured to the right, and current strength upwards. The unit of time is

$$\alpha = \frac{ckl^2}{10\pi^2} \log_e 10 = \frac{ckl^2}{42.86},$$

where l is the cable's length, and c, k its capacity and resistance

per unit of length. For a cable 2000 miles long, with $c = \frac{1}{3}$ microfarad, and $k = 6$ ohms, we have $ckl^2 = 8$ seconds, and $\alpha = .1866$ sec. The current, though calculable from the first instant, is relatively insensible for some little time. Thus, when $t = 1.5\alpha$, it has only reached .0047 of its final strength, but, thereafter increasing much more rapidly, reaches half its final strength in 6α , .98 in 20α , and its final strength $\frac{E}{kl}$ after the lapse of a theoretically infinite time.

Fig. 1.



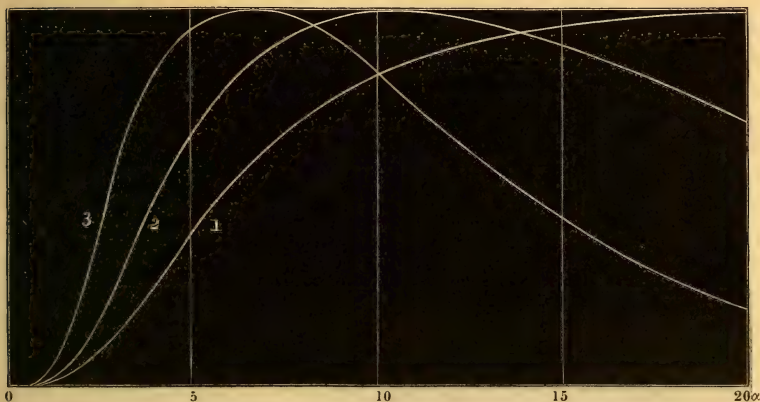
Now compare curve 1 with curve 3. In the latter all the circumstances are the same, with the exception that there is a fault of infinitely small resistance situated at the middle of the cable. Of course such a fault could not be worked through, since no current would arrive at the receiving end. Nevertheless this is not by any means a case of an unpractical nature; for it is quite possible to work, and very well too, a cable containing a fault of *next* to no resistance. It will be seen that with the fault of no resistance the current becomes sensible sooner, and increases much more rapidly. It reaches .0017 of its final strength in 1α , .044 in $1\frac{1}{2}$, .1318 in 2α , .4274 in 3α , .68 in 4α , .8357 in 5α , and .9826 in 8α . Half the final strength is reached in 3.3α , as against 6α with no fault.

When the fault has a finite resistance the arrival curve of the current is intermediate between curve 1 and curve 3. The one shown by curve 2 corresponds to the case of a fault having a resistance equal to one fourth of the cables. This makes the final strength of current $= \frac{E}{2kl}$, one half its value when there is no fault. In 2α the current reaches .0429 of

its final strength, half its final strength in 4.7α , and $.9845$ in 14α .

3. Now, referring to fig. 2, suppose both ends of the line to be insulated, and the cable free from charge. At any time $t=0$ let a small charge be instantaneously communicated to one end of the cable. This corresponds to working with condensers at both ends when the capacities of the terminal condensers are very small, and terminal resistance negligible. The charge thus communicated then diffuses itself along the cable, becoming finally equally distributed. Sir W. Thomson's mathematical theory indicates that the potential at any point x rises in exactly the same manner as the current rises at the same point when both ends of the line are to earth and a constant electromotive force operates at one end. Therefore the arrival curve of the potential at the distant end in working with condensers at both ends is the same as the arrival-curve of the current shown by 1, fig. 1. It is reproduced in 1, fig. 2, for comparison with the curves for a fault.

Fig. 2.



When there is a fault, or merely general loss through the insulator, there is conductive connexion between the conductor and earth; consequently the charge initially communicated to the beginning of the line must ultimately all escape, reducing the potential everywhere to zero. Therefore, although the current as shown by curve 1, fig. 1, never reaches its full strength, yet, since insulation is never absolutely perfect, the potential, as shown by curve 1, fig. 2, must sooner or later reach a maximum and then fall to zero. As the leakage increases, the time taken to reach the maximum decreases. The maximum is reached in 10.3α , as shown by curve 2, fig. 2, when there is a fault in the middle of the line of one fourth

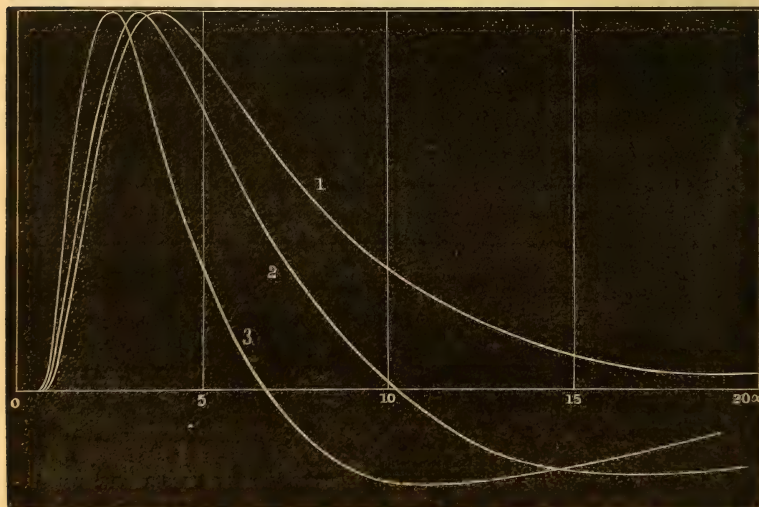
the resistance of the latter; and with a fault in the middle of infinitely small resistance, the maximum is reached in 6.5α , as shown by curve 3, fig. 2. It cannot be reached sooner with a single fault.

4. In condenser working, the working current is the current entering the receiving condenser—that is, the rate of increase of its charge, and therefore proportional to the rate of increase of the potential at the insulated receiving end when terminal resistances and capacities are neglected. The arrival-curve of the current when there is no fault is shown in fig. 3, curve 1. This curve was given by Sir W. Thomson in 1854, not however in connexion with condenser working (for that method was not then invented), but as showing the current at the distant end produced by an infinitely short contact with an infinitely powerful battery at the beginning, both ends being kept to earth. The current reaches a sensible proportion of its maximum much more rapidly than without condensers. It reaches its maximum in 3.93α , and then decreases.

With a fault of infinitely small resistance in the middle of the line, other things being the same, the arrival-curve of the current is shown by 3, fig. 3. It reaches $.0081$ of its maximum in $.8\alpha$, $.0523$ in 1α , and its maximum in 2.6α nearly. It then falls to zero, which it reaches in 6.5α , and becomes negative, as the electricity runs back to escape through the fault to earth.

Curve 2, fig. 3, is similar. It corresponds to the fault in the middle having one fourth the cable's resistance. The maximum is reached in 3.45α , and zero in 10.3α .

Fig. 3.



5. The influence of a fault on the amplitude of reversals may be readily calculated. In the first place, without condensers. Let contacts, alternately + and -, be made with a battery at the beginning of the line, while the distant end is to earth. If the reversals are sufficiently rapid, the resulting received current is nearly a simple harmonic function of the time. Let c be the capacity and k the resistance per unit of length of cable of length l , having a fault in the middle of resistance zkl . Also let τ be the period of a wave, or the time occupied by a pair of contacts. Then the maximum strength Γ of the received-current waves is

$$\Gamma = \frac{E}{kl} \cdot \frac{8n\sqrt{2}}{\pi} (\epsilon^n + \epsilon^{-n} - 2 \cos n)^{-\frac{1}{2}} \left\{ \epsilon^n + \epsilon^{-n} + 2 \cos n + \frac{1}{2nz} (\epsilon^n - \epsilon^{-n} + 2 \sin n) + \frac{1}{8n^2 z^2} (\epsilon^n + \epsilon^{-n} - 2 \cos n) \right\}^{-\frac{1}{2}},$$

where $n = \sqrt{\frac{ckl^2\pi}{\tau}}$, and E is the electromotive force of the battery. Or, approximately,

$$\Gamma = \frac{E}{kl} \cdot \frac{8n\sqrt{2}}{\pi} \epsilon^{-n} \cdot \left(1 + \frac{1}{2nz} + \frac{1}{8n^2 z^2}\right)^{-\frac{1}{2}}.$$

Here $\frac{E}{kl}$ is the current that would be produced in the line if perfectly insulated, and permanent contact made with the battery. $\frac{8n\sqrt{2}}{\pi} \epsilon^{-n}$ is the reversal-factor, and $\left(1 + \frac{1}{2nz} + \frac{1}{8n^2 z^2}\right)^{-\frac{1}{2}}$ the fault-factor. Now, if Γ_0 is the greatest current possible with the fault,

$$\Gamma_0 = \frac{E}{kl \left(1 + \frac{1}{4z}\right)};$$

therefore

$$\frac{\Gamma}{\Gamma_0} = \rho \cdot \phi(n),$$

where

$$\rho = \frac{1 + \frac{1}{4z}}{\sqrt{1 + \frac{1}{2nz} + \frac{1}{8n^2 z^2}}},$$

and $\phi(n)$ is the reversal factor. $\frac{\Gamma}{\Gamma_0}$ represents the proportion of the maximum received current which is arrived at by the

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reversals, or, for brevity, the proportional amplitude. If $z = \frac{1}{4}$,

$$\rho = \frac{2}{\sqrt{1 + \frac{2}{n} + \frac{2}{n^2}}}.$$

Let $n = 10$, which would make the time τ of a pair of contacts $\tau = \frac{ckl^2\pi}{n^2} = 1.347\alpha$, where α is the unit previously used ; then

$$\rho = \frac{2}{\sqrt{1.22}} = 1.82 \text{ nearly.}$$

Thus the fault increases the proportional amplitude for this speed 82 per cent. If $z = \frac{1}{100}$ and $n = 10$, then ρ is rather more than 6; and a fault of infinitely small resistance makes

$$\rho = \frac{n}{\sqrt{2}}.$$

6. Now for condenser working. Let every thing be the same as in the last paragraph, with the addition of a condenser of capacity $r_1 cl$ at the sending-end and another of capacity $r_2 cl$ at the receiving-end, r_1 and r_2 being extremely small. We shall now have

$$\Gamma = \frac{E}{kl} \cdot \frac{16n^3\sqrt{2}}{\pi} r_1 r_2 e^{-n} \left(1 + \frac{1}{2nz} + \frac{1}{8n^2 z^2}\right)^{-\frac{1}{2}}.$$

The fault-factor is the same as before ; and if the maximum received current possible were also the same as before, we should arrive at exactly the same conclusions as regards the influence of the fault on the proportional amplitude of the received current. But the comparison is here faulty, since

$$\frac{E}{kl\left(1 + \frac{1}{4z}\right)}$$

is the maximum current possible with both ends to earth, and the condensers do not allow the received current to reach such a strength, except in the imaginary case of condensers of infinite capacity ; for a condenser of infinite capacity is mathematically equivalent to a conductor of no resistance if there is no difference of potential between the coatings to start with, or to a battery of no resistance and electromotive force E if there is a difference of potentials E . But, as is shown later on, the maximum strength of the received current with condensers becomes proportional to

$$\frac{E}{kl\left(1 + \frac{1}{4z}\right)}$$

when z is very small ; so that for a fault of small resistance the same results follow as before for its effect on the proportional amplitude.

7. Since the proportional amplitude is increased by the fault for the same speed, a higher speed is obtained with the same proportional amplitude. Thus, with the ends of the cable to earth, as in paragraph 5, if n_1 is the value of n when there is no fault, then, to have the same proportional amplitude with a fault of resistance zkl in the centre, we must increase n to n_2 , so that

$$n_1 \epsilon^{-n_1} = n_2 \epsilon^{-n_2} \frac{1 + \frac{1}{4z}}{\left(1 + \frac{1}{2n_2 z} + \frac{1}{8n_2^2 z^2}\right)^{\frac{1}{2}}}.$$

Now the speed is inversely proportional to τ , and therefore directly proportional to n^2 ; therefore the percentage increase in the speed is

$$100 \left(\frac{n_2^2}{n_1^2} - 1 \right).$$

For $n_1=8, 9$, and 10 we shall find $n_2=9.7, 10.7$, and 11.7 , and the increase in the speed 47, 41, and 37 per cent., if $z=\frac{1}{32}$, which would make the greatest possible received current $\frac{E}{9kl}$.

For a fault of no resistance, $z=0$, and

$$n_1 \epsilon^{-n_1} = \frac{n_2^2}{\sqrt{2}} \epsilon^{-n_2}.$$

With $n_1=8, 9$, and 10 this gives $n_2=10.2, 11.3$, and 12.4 ; and the increase in the speed is 62, 57, and 53 per cent. These values of n_1 , namely 8, 9, and 10, are chosen on account of their nearness to the values in the working of long cables. The corresponding values of τ are 2.10, 1.66, and 1.34 α .

8. When a natural fault, or local defect in the insulation is developed in a cable, it tends to get worse—a phenomenon, it may be observed, not confined to cable-faults. Under the action of the current, the fault is increased in size and reduced in resistance, and, if it be not removed in time, ends by stopping the communication entirely. Hence the directors and officials of submarine-cable companies do not look upon faults with favour, and a sharp look-out is kept by the fault-finders for their detection and subsequent removal. But an artificial fault, or connexion by means of a coil of fine wire between the conductor and sheathing, would not have the objectionable

features of a natural fault. If properly constructed it would be of constant resistance, or only varying with the temperature, would contain no electromotive force of polarization, would not deteriorate, and would considerably accelerate the speed of working. The best position for a single fault would be the centre of the line; and perhaps $\frac{1}{2}$ of the line's resistance would not be too low for the fault.

9. In the cable, the potential v at any point x has to satisfy the differential equation

$$\frac{d^2v}{dx^2} = ck \frac{dv}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the current at x is

$$\gamma = -\frac{1}{k} \frac{dv}{dx}.$$

The particular solutions in paragraphs 5 and 6 regarding the strength of the received current when reversals are made with a battery at the sending-end are derived from the simple harmonic solution

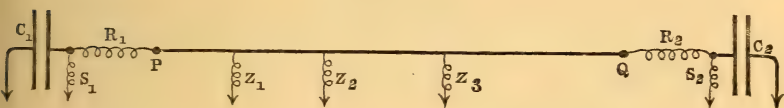
$$\begin{aligned} v = e^{\frac{nx}{l}} (A \cos + B \sin) \left(\frac{2\pi t}{\tau} + \frac{nx}{l} \right) \\ + e^{-\frac{nx}{l}} (A' \cos + B' \sin) \left(\frac{2\pi t}{\tau} - \frac{nx}{l} \right) \end{aligned}$$

of the above equation (1). When there are faults, each of the sections into which they divide the cable has a solution of the above form. In the case of a single fault, there are four conditions (namely, two for the fault and one for each end of the line) which suffice to determine the eight constants. But to determine the maximum strength of the received current, it is only necessary to find the sum of the squares of two of the constants. This shortens the labour, which is again greatly shortened by neglecting e^{-n} in comparison with 1.

10. The calculation of arrival-curves demands an entirely different method of proceeding. The general problem may be thus stated. Given a cable with faults in it, also the connexions at the ends, resistances, condensers, &c., and given also the electrical state of the whole system at a certain time: to find its state at any time after, the system being left to itself, and the action of the known laws regulating the potential, current, &c. In fig. 4 let P Q be a cable, of length l , resistance k , and electrostatic capacity c per unit of length. Also, let the terminal connexions be as shown, viz. at the beginning P a resistance R_1 and a condenser of capacity C_1 shunted by a resistance S_1 , with a similar arrangement at the end Q. This includes the cases of signalling either with or without

condensers, shunted or unshunted, at either or both ends. Let the signalling be from P to Q; then R_1 is the battery resistance, and R_2 the receiving-instrument's resistance. Let the electromagnetic capacity of the latter be L . Further, let there be n faults of resistances $Z_1, Z_2, \dots$ at distances $x_1, x_2, \dots$ from the beginning P, where $x=0$.

Fig. 4.



At the time $t=0$ let the potential of condenser C_1 be V_1 , and V_2 the potential of C_2 . Further, let $V=f(x)$ be the potential of the line when $t=0$. Since we have taken into account the magnetic capacity of the receiving-instrument, the specification of the initial state of the system is not complete unless we know the current in R_2 when $t=0$. Let this be G . Then we want to know v, v_1, v_2 , and g at time t , where v, v_1, v_2 , and g are what V, V_1, V_2 , and G then become.

11. Between any two faults let the initial potential be expanded in a convergent series of the form

$$\Sigma A \sin \left(\frac{ax}{l} + b \right).$$

This can be effected in an infinite number of ways. Then

$$\Sigma A \sin \left(\frac{ax}{l} + b \right) \epsilon^{-\frac{a^2 t}{T}}, \quad . \quad . \quad . \quad . \quad (2)$$

where $T=ckl^2$, satisfies the partial differential equation (1), and will therefore represent the potential at time t between the same limits, provided the sets of constants A, a , and b are so determined as to make (2) satisfy the conditions imposed by the presence of the faults and the terminal connexions. This, of course, can be done only in one way.

At each of the faults two conditions are imposed. First, the potential must be continuous at the fault; secondly, the current in the line going to the fault on the left side exceeds the current growing from the fault on the right side by the current in the fault itself from the conductor to earth; and the latter is, by Ohm's law, equal to the potential of the line at the fault divided by the resistance of the latter.

Let V_{0x_1} be the initial potential between $x=0$ and $x=x_1$, $V_{x_1x_2}$ between $x=x_1$ and $x=x_2$, and so on. Then the first

condition is satisfied at all the faults if

$$\left. \begin{aligned} V_{0x_1} &= \Sigma A \sin \left(\frac{ax}{l} + b \right), \\ V_{x_1x_2} &= V_{0x_1} + \Sigma B \sin \frac{a(x-x_1)}{l}, \\ V_{x_2x_3} &= V_{x_1x_2} + \Sigma C \sin \frac{a(x-x_2)}{l}, \end{aligned} \right\} \dots \dots \dots (3)$$

and so on. The second condition is satisfied at all the faults by making

$$\begin{aligned} B_i &= \frac{A_i}{z_1 a_i} \sin \left(\frac{a_i x_1}{l} + b_i \right), \\ C_i &= \frac{A_i}{z_2 a_i} \sin \left(\frac{a_i x_2}{l} + b_i \right) + \frac{B_i}{z_2 a_i} \sin \frac{a_i(x_2-x_1)}{l}, \\ &= \frac{A_i}{z_2 a_i} \left\{ \sin \left(\frac{a_i x_2}{l} + b_i \right) + \frac{1}{z_1 a_i} \sin \left(\frac{a_i x_1}{l} + b_i \right) \sin \frac{a_i(x_2-x_1)}{l} \right\}, \\ D_i &= \frac{A_i}{z_3 a_i} \sin \left(\frac{a_i x_3}{l} + b_i \right) + \frac{B_i}{z_3 a_i} \sin \frac{a_i(x_3-x_1)}{l} + \frac{C_i}{z_3 a_i} \sin \frac{a_i(x_3-x_2)}{l}, \end{aligned}$$

and so on, where

$$z_1 = \frac{Z_1}{kl}, \quad z_2 = \frac{Z_2}{kl}, \dots$$

12. The terminal arrangements have next to be considered. By the theory of the condenser, at the beginning P we have, at time t ,

$$-\frac{v_1}{S_1} - C_1 \frac{dv_1}{dt} = \frac{v_1 - v}{R_1} = -\frac{1}{k} \frac{dv}{dx}.$$

Hence, if

$$m_1 = \frac{R_1}{kl}, \quad n_1 = \frac{S_1}{kl}, \quad r_1 = \frac{C_1}{cl},$$

we have

$$v_1 = v - m_1 l \frac{dv}{dx} \quad \dots \dots \dots (4)$$

as the relation between the potentials of C_1 and the beginning of the line, and

$$0 = v - (m_1 + n_1) l \frac{dv}{dx} + n_1 r_1 l^2 \frac{d^2 v}{dx^2} - m_1 n_1 r_1 l^3 \frac{d^3 v}{dx^3} \quad \dots (5)$$

as the equation to be satisfied by the potential at $x=0$.

At the end Q we have

$$-\frac{1}{k} \frac{dv}{dx} = g = \frac{v_2}{S_2} + C_2 \frac{dv_2}{dt}, \quad \dots \dots \dots (6)$$

$$v - v_2 = g R_3 + L \frac{dg}{dt} \quad \dots \dots \dots (7)$$

Therefore if

$$m_2 = \frac{R_2}{kl}, \quad n_2 = \frac{S_2}{kl}, \quad r_2 = \frac{C_2}{cl}, \quad s = \frac{L}{kl \cdot ckl^2},$$

we have

$$v_2 = v + m_2 l \frac{dv}{dx} + sl^3 \frac{d^3v}{dx^3}, \quad . \quad . \quad . \quad (8)$$

giving v_2 in terms of v at $x=l$, and

$$0 = v + (m_2 + n_2)l \frac{dv}{dx} + n_2 r_2 l^2 \frac{d^2v}{dx^2} + (s + m_2 r_2 n_2)l^3 \frac{d^3v}{dx^3} + n_2 r_2 sl^5 \frac{d^5v}{dx^5} \quad . \quad . \quad . \quad (9)$$

as the condition held by the potential at $x=l$. In (5), (8), and (9), for $\frac{d}{dt}$ has been substituted $\frac{1}{ck} \frac{d^2}{dx^2}$.

13. Now the law of formation of the a 's and b 's can be found. From $x=0$ to $x=x_1$,

$$V_{0x_1} = \Sigma A \sin \left(\frac{ax}{l} + b \right);$$

and from the last fault at $x=x_n$ to $x=l$,

$$V_{x_n l} = \Sigma A \sin \left(\frac{ax}{l} + b \right) + \Sigma B \sin \frac{a(x-x_1)}{l} + \Sigma C \sin \frac{a(x-x_2)}{l} + \dots$$

Inserting the first of these in (5) and the second in (9), and then making $x=0$ in the first case and $x=l$ in the second, we find

$$\tan bi = \frac{(m_1 + n_1)a_i - m_1 n_1 r_1 a_i^3}{1 - n_1 r_1 a_i^2}, \quad . \quad . \quad . \quad (10)$$

$$\begin{aligned} & n(a_i + b_i) + q'_i \sin a_i \left(1 - \frac{x_1}{l}\right) + q''_i \sin a_i \left(1 - \frac{x_2}{l}\right) + q'''_i \sin a_i \left(1 - \frac{x_3}{l}\right) + \dots \\ & s(a_i + b_i) + q'_i \cos a_i \left(1 - \frac{x_1}{l}\right) + q''_i \cos a_i \left(1 - \frac{x_2}{l}\right) + q'''_i \cos a_i \left(1 - \frac{x_3}{l}\right) + \dots \\ & = - \frac{(m_2 + n_2)a_i - (s + m_2 n_2 r_2)a_i^3 + n_2 r_2 s a_i^5}{1 - n_2 r_2 a_i^2}, \quad . \quad . \quad . \quad (11) \end{aligned}$$

where

$$q'_i = \frac{B_i}{A_i}, \quad q''_i = \frac{C_i}{A_i}, \quad q'''_i = \frac{D_i}{A_i}, \text{ \&c.}$$

Equations (10) and (11) serve to determine the a 's and b 's.

14. Now only the A 's remain to be found. This is to be

effected by an integration along the line from $x=0$ to $x=l$, with a similar process applied at P and Q to the potentials of C_1 and C_2 and the current in R_2 . Collecting the expressions for the separate divisions, we have, from $x=0$ to $x=x_1$,

$$V_{0x_1} = \Sigma A_i \sin \left(\frac{a_i x}{l} + b_i \right) = \Sigma A_i M'_i, \text{ say; } \quad . \quad . \quad . \quad (12)$$

from $x=x_1$ to $x=x_2$,

$$\begin{aligned} V_{x_1 x_2} &= \Sigma A_i \left\{ \sin \left(\frac{a_i x}{l} + b_i \right) + q_i \sin \frac{a_i (x - x_1)}{l} \right\} \\ &= \Sigma A_i M''_i, \text{ say; } \quad . \quad (13) \end{aligned}$$

from $x=x_2$ to $x=x_3$,

$$\begin{aligned} V_{x_2 x_3} &= \Sigma A_i \left\{ \sin \left(\frac{a_i x}{l} + b_i \right) + q'_i \sin \frac{a_i (x - x_1)}{l} \right. \\ &\quad \left. + q''_i \sin \frac{a_i (x - x_2)}{l} \right\} = \Sigma A_i M'''_i, \text{ say; } \quad . \quad (14) \end{aligned}$$

and so on to the end of the line.

At the beginning P, by (4) we have

$$V_1 = \Sigma A_i (\sin b_i - m_1 a_i \cos b_i) = \Sigma A_i N'_i, \text{ say. } \quad . \quad . \quad . \quad (15)$$

At the end Q, by (8) we have

$$\begin{aligned} V_2 &= \Sigma A_i \left[\left\{ \sin (a_i + b_i) + q'_i \sin a_i \left(1 - \frac{x_1}{l} \right) \right. \right. \\ &\quad \left. \left. + q''_i \sin a_i \left(1 - \frac{x_2}{l} \right) + \dots \right\} + (m_2 a_i - s a_i^3) \left\{ \cos (a_i + b_i) \right. \right. \\ &\quad \left. \left. + q'_i \cos a_i \left(1 - \frac{x_1}{l} \right) + q''_i \cos a_i \left(1 - \frac{x_2}{l} \right) + \dots \right\} \right] \\ &= \Sigma A_i N''_i, \text{ say. } \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16) \end{aligned}$$

Also, let $V_3 = Gkl$; then by (6),

$$\begin{aligned} V_3 &= \Sigma A_i x - a_i \left\{ \cos (a_i + b_i) + q'_i \cos a_i \left(1 - \frac{x_1}{l} \right) \right. \\ &\quad \left. + q''_i \cos a_i \left(1 - \frac{x_2}{l} \right) + \dots \right\} = \Sigma A_i N'''_i, \text{ say. } \quad (17) \end{aligned}$$

To find A_i , the i th value of A . Multiply both sides of each one of the last equations, (12) to (17), by the coefficient of A_i in that particular equation; *e.g.* multiply (12) by M'_i , (13) by M''_i , and so on. Next integrate each side belonging to the line between the limits for which it is true. Thus (12) from $x=0$ to $x=x_1$, &c. Apply a similar process to V_1 , V_2 , and

V_3 by multiplying them by $r_1 l$, $r_2 l$, and sl respectively. Finally add together all the results, right and left sides respectively, excepting for V_3 , which must be subtracted, and then equate the two sums. The result is

$$\begin{aligned} & \int_0^{x_1} V_{0x_1} M'_i dx + \int_{x_1}^{x_2} V_{x_1 x_2} M''_i dx + \int_{x_2}^{x_3} V_{x_2 x_3} M'''_i dx + \dots \\ & + V_1 r_1 l N'_i + V_2 r_2 l N''_i - V_3 s l N'''_i = \sum_{i'=0}^{i'=\infty} \left\{ \int_0^{x_1} A_{i'} M'_i M'_{i'} dx \right. \\ & + \int_{x_1}^{x_2} A_{i'} M''_i M''_{i'} dx + \int_{x_2}^{x_3} A_{i'} M'''_i M'''_{i'} dx + \dots + A_{i'} r_1 l N'_i N'_{i'} \\ & \left. + A_{i'} r_2 l N''_i N''_{i'} - A_{i'} s l N'''_i N'''_{i'} \right\}. \quad \dots \quad (18) \end{aligned}$$

It will be found, on making the substitutions in (18) of the expressions for the M 's and N 's, and effecting the necessary reductions, that in the summation on the right-hand side of (18), the complete coefficient of every one of the A 's vanishes identically, by reason of equations (10) and (11), *except* for A_i ; whence

$$\begin{aligned} A_i = & \frac{\int_0^{x_1} V_{0x_1} M'_i dx + \int_{x_1}^{x_2} V_{x_1 x_2} M''_i dx + \dots + V_1 r_1 l N'_i + V_2 r_2 l N''_i - V_3 s l N'''_i}{\int_0^{x_1} M_i'^2 dx + \int_{x_1}^{x_2} M_i''^2 dx + \dots + r_1 l N_i'^2 + r_2 l N_i''^2 - s l N_i'''^2} \\ & \dots \quad (19) \end{aligned}$$

This completes the solution; and the state of the whole system is determined for any time t .

15. When the initial potentials V_{0x_1} &c. of the line in the different sections are given explicitly as functions of x , the sum of the integrals in the numerator of (19) may be written

$$\begin{aligned} & \int_0^l V_{0x_1} \sin \left(\frac{a_i x}{l} + b_i \right) dx + \int_{x_1}^l V_{x_1 x_2} q'_i \sin \frac{a_i (x - x_1)}{l} dx \\ & + \int_{x_2}^l V_{x_2 x_3} q''_i \sin \frac{a_i (x - x_2)}{l} dx + \dots \end{aligned}$$

There is a great simplification when the initial state of the system is, not arbitrary, but such as would be finally produced by a constant electromotive force E acting at P (fig. 4). Then the complete numerator of (19) reduces to

$$\frac{El \cos b_i}{a_i}$$

for any number of faults and for all the terminal arrangements that can be made out of those shown in fig. 4. The denominator of (19) is a function of a_i and b_i . Thus

$$A_i = \frac{El \cos b_i}{a_i \phi(a_i b_i)} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (20)$$

[To be continued.]

VIII. On the Size of Molecules. By N. D. C. HODGES*.

IF we consider unit mass of water, the expenditure on it of an amount of energy equivalent to $636\cdot7$ units of heat will convert it from water at zero into steam at 100° . I am going to consider this conversion into steam as a breaking-up of the water into a large number of small parts, the total surface of which will be much greater than that of the water originally. To increase the surface of a quantity of water by one square centimetre requires the use of $\cdot000825$ metre-gramme of work. The total superficial area of all the parts, supposing them spherical, will be $4\pi r^2 N$, the number of parts being N . The work done in dividing the water will be $4\pi r^2 N \cdot 000825$. For the volume of all the parts we have $\frac{4}{3}\pi r^3 N$. This volume is, in accordance with the requirements of the kinetic theory of gases, about $\frac{1}{3000}$ of the total volume of the steam. The volume of the steam is 1752 times the original unit volume of water. Hence—

$$\begin{aligned} \frac{4}{3}\pi r^3 N \cdot 3000 &= 1752 \\ 4\pi r^2 N \cdot 000825 &= 636\cdot7423 \end{aligned}$$

One unit of heat equals 423 units of work.

Solving these equations for r and N , we get $r = \cdot000000005$ centimetre, a quantity closely corresponding with the previous results of Sir William Thomson, Maxwell, and others; and N equal 9000 (million)³, or for the number in one cubic centimetre 5 to 6 (million)³.

Around every body there is an atmosphere of more or less condensed gases. On the surface of platinum these must be nearly in the liquid condition, as shown by the power of platinum to bring the atoms of oxygen and hydrogen so near together that they combine. These vapours on the surface have a tendency at ordinary temperatures to expand; and part of them can do so, if the surface of the body is reduced. There is in these condensed atmospheres an explanation of all the phenomena of superficial tension. The energy in the unit of area ought to be equal to the work done in compressing a

* Communicated by the Author.

quantity of the vapour from the gaseous to the liquid state sufficient to cover the surface a few molecules deep. The molecular attraction seems to be very slight in gases, where the molecules are ten or fifteen molecular diameters apart. To get some idea of the amount of work done in compressing one gramme of oxygen to the liquid state, we may consider that in the union of one gramme of hydrogen with eight grammes of oxygen 34,462 units of heat are produced. It matters not that the condensation is brought about by the energy of chemical separation rather than by the work done in pressing them together in a cylinder.

The superficial energy of platinum is 169·4 metre-grammes per square metre, or ·01694 per square centimetre, equal to ·00004 of a unit of heat. The proportion

$$9 : 34,462 = x : \cdot 00004$$

gives the weight of water condensed on one square centimetre of surface, or the volume in cubic centimetres as ·00000001, which is the thickness of the layer, or diameter possibly, of the molecules.

Physical Laboratory, Harvard College,
May 14, 1879.

IX. *On a new Form of Spectrometer, and on the Distribution of the Intensity of Light in the Spectrum.* By JOHN WILLIAM DRAPER, M.D., *President of the Faculty of Science in the University of New York**.

I HAVE invented a spectrometer which, I think, opens a new and interesting field to those who are engaged in spectrum-analysis.

The ordinary spectroscope is occupied with the frequency of æther-vibrations or wave-lengths. That which I am about to describe has a different function. It deals with the intensity or brilliancy of light. It depends on the well-known optical principle that a light becomes invisible when it is in presence of another light about sixty-four times more brilliant.

In some researches published by me in 1847, on the production of light by heat, or the incandescence of bodies, I used this method as a photometer, and became sensible of its value. The memoir in which those experiments are related may be found in my recently published 'Scientific Memoirs,' page 23.

Having also published in 1872 a memoir on the distribution of heat in the prismatic spectrum, and shown that the course

* Communicated by the Author.

of its increasing intensity from the more to the less refrangible regions is due to the compression of the coloured spaces that correspondingly takes place, owing to the action of the prism itself, but having failed to obtain satisfactory measures in the case of the diffraction-spectrum (in which such compression or condensation does not occur), I was led to reflect whether better success might not be secured by attempting to measure the relative intensity or distribution of light.

Admitting what is commonly received as true, that the yellow is the brightest of the coloured spectrum spaces, and that the luminous intensity diminishes from that in both directions above and below, I supposed that, if such a spectrum were brought into the presence of an extraneous light, the illuminating power of which could be varied at pleasure, after the red and the orange on one side, and the green, blue, indigo, and violet on the other, had been extinguished, the yellow would still remain in the midst of the surrounding illumination. On making the experiment it turned out differently.

For the sake of clearness of description I will call this extraneous light, from the functions it has to discharge, *the extinguishing light*.

There are many different plans by which the principle above indicated may be carried into practical effect. Several of these I have tried, and have found the following a convenient one.

Remove from the common three-tubed spectroscope its scale-tube, and place against the aperture into which it was screwed a piece of glass ground on both sides. In front of this arrange an ordinary gas-light, attached to a flexible tube, so that its distance from the ground glass may be varied at pleasure. On looking through the telescope, the field of view will be seen uniformly illuminated, this being the use of the ground glass. The brilliancy of the field depends on the distance of the gas-light, according to the ordinary photometric law.

1. *Case of the Prismatic or Dispersion Spectrum.*

If the extinguishing light be for the moment put out, and in the proper place before the slit-tube the *luminous* flame of the Bunsen burner that accompanies the apparatus be arranged, on looking through the telescope a spectrum of that luminous flame will of course be seen. The slit itself should be very narrow, so that the spectrum may not be too bright.

Now let the extinguishing flame be placed before the ground glass, and a spectrum is seen in the midst of a field of light, the brilliancy of which can be varied at pleasure. If this ex-

tinguishing flame be at a suitable distance, the whole spectrum may be discerned. As that distance is shortened, first the violet and then the other more refrangible colours in their descending order disappear, and at length in the steadily increasing effulgence the red alone remains. The yellow never stands out conspicuously, as might have been expected.

This is scarcely consistent with the assertion that the yellow is the brightest of the rays. The red is plainly perceptible long after the yellow has gone. There is a greenish tint emitted by gas-light that disappears a little previously to the extinction of the red.

From these observations I think that the luminous intensity of the coloured spaces has a relation to the compression or condensation that the prism is impressing upon them. It may be that, properly considered, the intrinsic intensity of the light is the same for all. In this we must always bear in mind the physiological peculiarities of the eye.

The foregoing statement is perhaps sufficiently explicit to enable any one to verify the facts. I may, however, mention some improvements in the apparatus, which experience has led me to adopt.

The intensity of the extinguishing light may be insufficient to obliterate the spectrum even though the slit be closely narrowed. How, then, may the intensity of the spectrum be diminished, and that of the extinguishing light be simultaneously increased? I accomplished this by depositing on that face of the prism which acts as a reflector an *excessively* thin film of silver. This, though it was to the transmitted rays quite transparent, increased very greatly by its metallic reflection the extinguishing ones. I could not see any difference between the spectrum of the light that had come through this film and that before the face was silvered, but the reflected light was incomparably more brilliant. The complete obliteration of the entire spectrum presented now no difficulty.

Nothing need be said about collateral contrivances, which would suggest themselves to any one. A strip of wood a metre long and bearing divisions, served to keep the extinguishing lamp in the proper direction as regards the ground glass, and indicated its distance. I may add, however, that satisfactory observations can be made very conveniently by keeping the extinguishing flame at a constant distance, and varying its intensity by opening or closing the stopcock. This avoids the trouble arising from moving it. In one instrument I caused an index attached to the head of the stopcock to move over a graduated scale, and so ascertained how much it was opened.

This, though permitting of pleasant working, had not the exactness of the method of distances.

Such are the results obtained from the prismatic dispersion of gaslight. I completed this part of the investigation by an examination of sunlight. For this purpose I resorted to the foregoing principle, introducing a beam of sunlight reflected from a heliostat through a slit. The spectrum of this was thrown upon a paper screen, so placed that by opening or closing an adjacent window-shutter the light of the sky in greater or less quantity could fall upon the paper, and act as an extinguisher. When the shutter was fully opened, the spectrum was quite obliterated; and on gradually closing it so as to diminish the extinguishing light, the red region first came into view, the other colours following in the order of their refrangibility, the extreme violet appearing last. On reversing the movements of the shutter the colours disappeared in the reverse order, the red disappearing last.

At the moment when the red was approaching extinction there always existed on its more refrangible side a gleam of greyish-green light. In was in the position of that greenish gleam which appeared, as I have described, when gaslight was examined. Its colour recalled to my mind the faint greenish-grey light I had seen when a strip of platinum was ignited by a feeble electric current, as described in my memoir of 1847, above referred to.

Subsequently I constructed a camera having two apertures in its front. Through one of them (by a suitable arrangement of a heliostat, slit, direct-vision prism, and convex lens) a solar spectrum was formed on the ground glass. Through the second aperture, which was about an inch square, covered with a glass ground on both faces, an extinguishing beam of sunlight passed. This ground glass served to disseminate the extinguishing light uniformly over the spectrum. I could regulate its power by varying the size of the aperture through which it came, by means of a slide.

It is needless to give details of the results obtained by this instrument. They were identical with those described in the foregoing paragraphs.

It might be supposed that the irrationality of dispersion of different prisms would influence the results perceptibly. Accordingly I tried prisms of different kinds of glass and other transparent substances, but could not find that this was the case. In all the extinction began in the violet and ended in the red.

Nor did there seem to be any difference when the effect was viewed by different eyes. To all, irrespective of age or

the condition of their sight, the extinction took place in the same manner. I had not an opportunity of examination in a case of colour-blindness.

2. Case of the Grating or Diffraction-spectrum.

If the cause of the increasing intensity of light, in the prismatic spectrum, from the more to the less refrangible region, be the compression exercised by the prism on the coloured spaces, increasing as the refrangibility is less, we ought not to find any such peculiarity in the diffraction-spectrum. In this the coloured spaces are arranged uniformly and equally in the order of their wave-lengths. An extinguishing light ought to obliterate them all at the same moment.

Having modified the common three-tube spectroscope, as has been described, I put in the place of its prism a glass grating inclined at 45° to rays coming in through the slit. The ruled side of the grating was presented to the slit. Now, when the extinguishing flame was properly placed before the ground glass, the plain side of the grating reflected its light down the telescope-tube. In this, as in the former case, the spectrum was seen in the midst of a field of light, the intensity of which could be varied by varying the distance of the extinguishing flame, or by varying the opening of its stopcock. This light needs no reinforcement by increasing the reflecting power of the back face of the grating, these spectra being much more feeble than those given by a prism, and the unassisted light being quite able to extinguish them.

As the glass grating I was using gave its two series of spectra of unequal brightness, I selected the most brilliant, and in it used the spectrum of the first order. I saw, not without pleasure, that as the force of the extinguishing illumination increased, all the coloured spaces yielded apparently to an equal degree, and disappeared at the same moment. Sometimes, however, there seemed to be a very slight difference in favour of the red. On diminishing the illumination all the colours came into view, apparently at the same time. This spectrum gives a better opportunity than the prismatic for observations on the yellow space, which, by being uncompressed, exposes a wider surface to view. This yellow space showed no superiority in resisting extinction over the other colours.

But as gaslight compared with sunlight is deficient in the more refrangible rays, I repeated the examination of the latter, as I had previously done for the prismatic spectrum,

modifying the apparatus so as to use a grating in place of the prism. The observations in this case of sunlight were quite as satisfactory as those in which gaslight had been used.

General Conclusions.

1st. In the prismatic spectrum the luminous intensity increases from the more to the less refrangible spaces, its maximum being not in the yellow but in the red. This is due to the action of the prism, which narrows and as it were condenses the coloured spaces more and more as we pass towards the red, increasing the intensity of the light as it does that of the heat.

2nd. In the grating or diffraction-spectrum the luminous intensity is equal in all the visible regions, all the colours being simultaneously obliterated by an extinguishing light.

It must however be borne in mind that these conclusions should be taken in connexion with the physiological action of the eye. Owing in part to the imperfect transparency of its media, and partly to the inability of its nervous mechanism to transmit waves of certain frequency to the brain, the spectrum does not begin and end sharply, as to a perfect eye a perfect spectrum ought to do.

There are hence two causes which must not be overlooked in these observations. 1st, the physiological peculiarity of the eye, which gives to each end of the spectrum the aspect of gradually fading away; 2nd, in the case of solar light the absorption action of the atmosphere, which is chiefly exerted on the more refrangible rays.

I think, bearing in mind the correlation of light and heat, both being corresponding manifestations of the same vibrating movement in the æther, that these results substantiate those I published in 1872 on the distribution of heat in the spectrum, and that as the different coloured spaces are equally luminous, so they are equally warm.

I have made some attempt to compare with each other the luminous intensity of the bright lines in various spectra, especially those emitted by a strontium-flame; but not being able to continue these researches at present, I have postponed them to a more favourable opportunity.

University of New York,
May 5th, 1879.

X. *Intelligence and Miscellaneous Articles.*

ON THE ELECTRIC LIGHT. BY J. JAMIN.

I HAVE the honour to present to the Academy a pattern of electric burner reduced to the simplest form possible. The two carbons are maintained parallel by two insulated copper tubes separated by an interval of 2 or 3 millims., in which they slide with friction, and which serve at the same time to direct them and to bring the current. They are surrounded by a directing circuit composed of five or six helices coiled upon a thin rectangular frame 40 centims. in length and 15 centims. wide. I have explained how this circuit, traversed by the same current as the carbons and in the same direction, brings and fixes the electric arc at the extremity of the points.

The lighting takes place automatically. For this purpose the two extremities of the carbons are wrapped round with a thin caoutchouc band, which presses them to one another; a small piece of iron wire is then insinuated between them, or a little above, which puts them into communication at one point only. As soon as the circuit is closed the current passes through this wire, makes it red-hot, and melts the caoutchouc; the two carbons, having again become free, separate, and the arc is set up with a sort of explosion. Carbons can be employed of any size up to 8 millims. diameter. At this limit the wear does not exceed 8 centims. per hour. As it goes on, the points approach the supporting tubes; but they can be from time to time brought back to their initial position by sliding the carbons in the tubes by a common movement, without extinguishing them. In future applications an easily devised mechanism will perform this office; and as M. Carré manufactures carbons the length of which reaches 1 metre, the lamp can remain lighted during twelve hours, which exceeds all needs. It will be remarked that the carbons are not separated by any insulating material, that it is not necessary to first break off their points, nor to fix them at their base, nor to furnish them at their point with any inflammable material; they are used just as they come from the maker's. It is sufficient to introduce them into the tubes which are to support them, and to leave them to the directing action of the outer circuit. In reality there is no candle to construct; there is only a sort of wick to place, which burns all alone to the end.

The apparatus can be suspended in two ways—either with the points above, or directing them towards the ground. These are very different conditions. Let us study the first case.

The electric arc cannot, without breaking, exceed a length depending on the intensity of the current. Between two horizontal points it should be rectilineal, because, according to the laws of conductivity it takes the shortest path, and tends to return to it when forced to deviate, in virtue of a sort of elasticity. But it is deranged by the ascending currents of air occasioned by its heat; and that is why it takes a curved form. It is also deranged, and much more energetically, by the directing circuit. These two actions join in bending it upwards until equilibrium is established between them and its elasticity; but they also combine to lengthen

it, and to diminish at the same time its resistance to breaking and the intensity of the current. It is evident that if they cooperate to fix the light at the summit of the carbons, it is on the condition of lessening the extreme length the arc can attain, or, what is the same thing, the number of foci that can be kept alight with a given machine.

It is no longer so when the points are turned towards the ground. While the arc tends to ascend along the carbons, the directing circuit drives it back, lowers it, and lodges it between the points, distant from 7 to 8 millims. The two actions, which before were added together, are now subtracted one from the other; and so far from lengthening the arc, they shorten it; instead of diminishing its resistance to rupture, and lessening the intensity of the current, they augment both. The arc may be said to be as it were compressed between two opposite actions: it is shorter, narrower, less expanded, more dense, and consequently hotter; and the number of the foci can be augmented. M. Jabloschkoff's candles, in other respects so well combined, have nevertheless the inconvenience that the points are directed upwards. The arc produced by them has a natural tendency to bend and raise itself; and the same tendency is impressed upon it by the electromagnetic action exerted upon it by the current, which ascends in one carbon and descends in the other—an action identical with that of my directing circuit, though less energetic. The burner with the points below must therefore excel those candles. In fact this is proved by experience. With a machine hardly sufficient to light three of those candles I can easily maintain five burners armed with much bigger carbons, each giving about twice as much light; and as the points are immersed in the mass of the arc, they acquire a more vivid brightness and an incomparably whiter tint. Six foci, even, can be lighted; but they give a less sum total of light than five; if we double the number we lose in quantity. It is always so when the electric light is divided beyond measure: the division must be purchased by a proportional loss.

The management of these burners requires careful study. When the points are above, the lighting is very difficult, because immediately it is produced the force of the directing current projects it strongly upwards, that force being proportional to the square of the intensity. When this increases, it becomes absolutely impossible to light the carbons; one can get only a vast flame which immediately disappears with a noise. If the current is less intense, the light continues, but much spread out, very tall, and always very noisy, on account of the amplitude of the oscillations which take place at each inversion of the current. Finally, the equilibrium is not at all stable. If an accidental current of air comes for a moment to augment the height of the flame, nothing can restore it, the limit of its elasticity is passed, and it soon breaks. In the burners with the points beneath, the lighting is easy, and the equilibrium stable; for if a movement of the air or a deficiency of the current causes the arc to ascend, it settles between the two carbons at the place where they have not been thinned by combustion; it lodges in an interval not exceeding 2 or 3 millims. Far

from lengthening, it shortens; instead of decreasing, its resistance to rupture and the intensity augment; and the light gently redescends to resume and keep its place at the extremity of the carbons. If, on the contrary, the current should increase, the arc bends and becomes concave towards the carbons; but, its tendency to ascend counterbalancing the action of the directing current, it never stretches enough to break. The best economic conditions are attained when this curve is just pronounced enough to prevent the ascensional movement of the light. In this case the unavoidable noise of the electric light is reduced to its minimum, because the amplitudes of the vibratory motion are the smallest possible.

In brief, the burner which I submit to the Academy, with its points beneath, realizes considerable advantages:—(1) that of simplicity, since it needs no mechanism and requires no preliminary preparation; the whole amounts to a support and some carbons; (2) that of mechanical economy, since the number of the flames is almost doubled; (3) increase of light, since each of the new foci is nearly twice as effective as the old ones; (4) the quality of the light, which is whiter; (5) a more advantageous disposition of the foci, which direct their greatest sum of light downwards, where it is of use, instead of losing it skywards, where it would be useless; (6) lastly, economy of the combustible, since in proportion to the size of the carbons the consumption is less. All this constitutes for the electric light a sensible advance, and cannot fail to enlarge the place it has already taken in public lighting, thanks to the improvement of the engines, to the carbons of M. Carré, and to M. Jabloschkoff's candle.—*Comptes Rendus de l'Académie des Sciences*, April 28, 1879, t. lxxxviii. pp. 829-832.

ON THE DEVIATIONS OF AMPÈRE'S THEORY OF MAGNETISM FROM
THE THEORY OF THE ELECTROMAGNETIC FORCES. BY J.
STEFAN.

The divergences between the two theories stand out most clearly when the moments with which a current-element tends to rotate an elementary magnet are determined according to both. According to the electromagnetic theory, the current-element rotates the magnet out of the plane when both stand perpendicular to the line joining them and are situated in the same plane; while according to Ampère it cannot act at all upon the elementary current equivalent to the magnet, since it stands perpendicular to every part of the latter and at the same time to the connecting lines leading to them.

If the current-element falls into the connecting line, according to the electromagnetic theory it exerts no rotating action upon the magnet; but according to Ampère it sets the equivalent elementary current continuously rotating about its axis—an action which is quite foreign to the electromagnetic theory.

If the magnet lies in the direction of line of conjunction, a current-element perpendicular to this, according to both theories, rotates the magnet out of the plane, but according to Ampère's theory with a moment twice as great as according to the electromagnetic.

For closed circuits the differences compensate one another, but the parts which the individual elements of the circuit have in the total action are different. Thus, according to the electromagnetic theory the elements of the vertical circuit of a tangent-compass all act with equal deflecting force upon the very short needle; according to Ampère the elements situated in the vertical diameter exert no action, while those in the horizontal diameter act twice as powerfully as according to the other theory.

In the memoir the electromagnetic is compared with the general electrodynamic theory, which also assumes transversally-acting forces, and was developed by the author in his memoir "On the Fundamental Formulæ of Electrodynamics," published in vol. lix. of the *Sitzungsberichte* of the Academy (1869).

The comparison has reference, first, to the forces, and, secondly, to the pairs of forces, which a current-element exerts upon an elementary current. In the former respect Ampère's theory, and all those which also assume transversal forces, though of such a nature that they cannot move the common centre of gravity of two current-elements, agree with the electromagnetic theory. In the second respect the electromagnetic theory corresponds only with Grassmann's. But as these two theories differ in the expressions for the forces, there is not any electrodynamic theory containing in itself the electromagnetic. With the exception of Grassmann's, all the theories contain, though not in like manner, continuous rotations of the magnets by the action of the component of the current-element in the connecting-line.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, April 17, 1879, pp. 110, 111.

ON THE PRODUCTION OF BARIUM FROM BARIUM-AMALGAM.

BY JULIUS DONATH, OF GRAZ.

In this note we are informed that the amalgamated barium easily obtained by Crookes's method, by digesting solution of barium chloride with sodium-amalgam, *does not* after distillation contain any pure barium at all, but leaves only an amalgam rich in barium, which may contain 62–77 per cent. of mercury.

It was already mentioned by Bunsen that the barium- or calcium-amalgam that appears at the amalgamated platinum electrode in the electrolysis of aqueous solution of chloride of barium or calcium respectively, also obstinately retains mercury.

In all probability S. Kern's method, viz. heating oxide and iodide of barium with sodium, extracting with quicksilver, and distilling, will yield no better result. All the statements about the *silvery-white metallic lustre* of barium must evidently be referred to the amalgam. Pure barium, as obtained by Bunsen and Matthiessen by electrolysis of the fused chloride, is bronze-coloured. This colour, it is true, is sometimes exhibited by the most superficial layers of the barium-rich amalgam, which under the action of a very elevated temperature have been deprived of mercury; but even such portions of the amalgam are grey on the inside, and in water they leave considerable traces of mercury.—*Kaiserl. Akad. d. Wissensch. in Wien, math.-naturw. Classe*, 1879, No. X. p. 109.

THE
LONDON, EDINBURGH, AND DUBLIN
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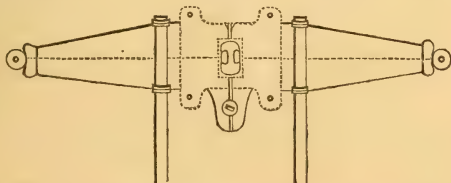
[FIFTH SERIES.]

AUGUST 1879.

XI. *Electro-optic Observations on various Liquids.* By JOHN KERR, LL.D., *Free Church Training College, Glasgow*.*.

IN two short papers which were published some years ago, I showed how I had succeeded in inducing a power of double refraction in glass, carbon disulphide, and several other dielectrics, by the application of electric force†. In this paper I propose to offer some notes of a later and more extended series of experiments on the same subject. The methods applied are, for the most part, much the same in principle now as formerly; but my means of observation have been greatly amplified and improved, chiefly by assistance from the Government Fund. I begin with the construction of the most important part of the apparatus.

1. *New Plate Cell.*—This piece is represented in the adja-



cent diagram. It is made of a block of carefully selected

* Communicated by the Author.

† "On a new Relation between Electricity and Light," *Philosophical Magazine*, November and December 1875.

plate glass, $\frac{3}{4}$ of an inch thick, 8 inches long, and originally, for convenience in boring, about 4 inches wide. The first step in the construction is made with steel drill and turning-lathe. Two fine holes, about $\frac{1}{10}$ of an inch wide, are drilled right through the block, one parallel to its length, and the other crossing the former at right angles in the centre of the piece. Each of the borings is parallel to, and equidistant from, the two plate-faces.

The plate is now reduced to a more convenient width, one inch of it (as it stands in the diagram) being ground away at the top from end to end, and similarly one inch at the bottom, except that a piece with sensibly square section is left projecting below the plate, round the vertical boring as axis. The plate is also made to taper at each end, as in the diagram, though this is not essential.

Two other borings are now made, each through the plate, at right angles to the plate-faces. One is a tunnel, concentric with the plate, shaped as in the diagram, about an inch in height, and $\frac{5}{8}$ inch in width, and leaving a good margin of polished plate-surface all round its mouths; the other is a slightly tapering hole through the projecting piece mentioned above, into which is fitted afterwards a stopcock of glass, which is easily worked by hand so as to open and close the vertical boring. The sides of the tunnel are carefully finished; they are sensibly plane, and perpendicular to the long boring and to the plate-faces. In these and the following operations, the polish of the plate is preserved with care, for which purpose the surfaces are permanently guarded by a shell of hard varnish.

The electric terminals within the tunnel are two balls of brass, originally spherical, and a quarter inch in diameter. Two thin shafts of brass pass from the ends of the block through the long borings, and are screwed firmly into the balls. Round the outer end of each of the shafts is a pierced plug or washer of india-rubber; outside of this is a perforated disk of brass, of rather smaller diameter than the washer, and well rounded, which moves freely along the shaft; and outside of each disk is a brass ball of $\frac{1}{2}$ inch diameter, which screws onto the end of the shaft. To provide for the insertion of conducting wires into these outer balls, two fine holes are bored through each of them, along diameters perpendicular to each other and to the shaft. The outer balls are screwed along the shafts until the washers are very strongly compressed. To prevent all possibility of leakage at the junction of inner balls and block, each of the long borings has been widened a little at the inner

end into a conical funnel; each of the inner balls also has been backed by a zone of lead.

At this stage of the construction, when the small spheres were seen to be symmetrically and securely placed in the tunnel, they were taken out, and very carefully flattened in the turning-lathe, so as to present approximately plane but still well rounded faces to each other; and they were then electroplated with a shell of silver as thick as writing-paper. As the balls lie finally in the cell, their least distance from each other is exactly $\frac{1}{8}$ inch.

The cell is closed by two panes of the finest plate glass, about $\frac{1}{16}$ inch thick, and 2 inches square, which are simply kept upon the plate faces of the block by pressure. The press is made of two small planks of mahogany, shaped as shown by the dotted lines in the diagram, and connected at the corners by four square-headed screw-nails, each provided with a bat's-tail nut; but in working the cell, I find two diagonally opposite screws quite sufficient in most cases. Between the planks and glass are two thin sheets of india-rubber cloth, these, as well as the planks, being channelled neatly in continuation of the tunnel.

The whole piece is supported by two fine pillars of glass, which are firmly fastened to the block by coils of silk thread. The pillars terminate below in a solid wooden stand. When the cell is in position on the experimental table, the two faces of the plate of liquid are vertical, and the axis of the electric field, or the straight line joining the centres of the inner balls, is horizontal.

The supporting pillars, as well as the ends of the cell-block, are covered with a thick shell of lac varnish.

I think that I have now mentioned every thing essential to the cell, except a small stopper of glass, which, at the beginning of each experiment, is dropped into the mouth of the vertical boring. It prevents the entrance of stray particles of dust, and is of use also in restraining evaporation when the cell is charged with a very volatile liquid. This plate cell is far superior to the old one described in the second of my former papers; it gives finer optical effects, and is ever so much more easily handled.

2. *Working of the Cell.*—To charge the cell with a liquid, I always use a small filtering funnel drawn out into a fine end, which passes along the upper boring, quite through the roof of the tunnel, leaving a narrow surrounding space for escape of air. When the charge of liquid is found to be not sufficiently clean, the cell is emptied at once by turning the lower cock, and is then charged again from the bottle as before; and

a good many repetitions of this operation are sometimes required before the charge is sensibly free from specks of solid matter.

When the cell has to be charged with a new liquid, the press is unscrewed, the planks and panes are removed, the faces of the block are cleaned, two clean panes are laid against the mouths of the tunnel, and held there while the cell is thoroughly rinsed with a proper solvent. The panes are removed, the faces of the block are again cleaned, two clean panes are placed as before, and the rinsing is repeated. Half-a-dozen thorough rinsings with ether and alcohol are generally sufficient to prepare the cell perfectly for any new liquid.

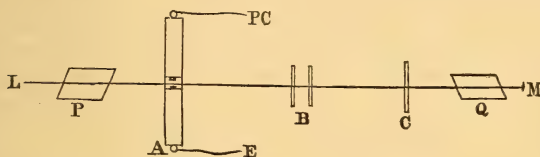
3. *Optical Compensator of Strained Glass*.—In the following experiments, I require continually to introduce definite and very faint birefringent actions at some point between polarizer and analyzer. The pieces which I employ for this purpose are perfectly rectangular slips, all cut out of one carefully selected plate of glass $\frac{1}{16}$ of an inch thick, and all of the same dimensions, width $\frac{3}{8}$ of an inch, length 7 inches. When such a slip is extended, it acts upon the transmitted light as a positive uniaxal with axis along the line of tension; and when it is compressed it acts as a negative uniaxal with axis along the line of compression. These optical actions are in most cases very definite and sensibly pure, though perhaps never perfect as the actions of good natural crystals.

4. *Hand Compensator*.—This is one of the slips (3), worked by hand in the manner described in my former papers. It is held between the first and second Nicols, immediately in front of the latter, with its faces perpendicular to the ray, and its length inclined at 45° to the plane of polarization. Kept steadily in position, it is strained by an effort of hands or fingers at each end, the axes of the couples applied being parallel to the ray, so that one edge of the slip is extended and the opposite edge compressed. According to the statement already made in (3), the extended and compressed parts of the slip act respectively as positive and negative uniaxals with axes parallel to the edges. When the glass is chosen with ordinary care, and preserved from any considerable changes of temperature during the observation, these optical actions are (to sense) perfectly pure, tension of one slip being always totally neutralizable by parallel compression or perpendicular tension of another slip.

5. *Fixed Compensator*.—This is one of the standard slips (3), which hangs freely in a constant vertical position from a purposely constructed stand. To give the required effect of

double refraction, the slip is simply stretched along its length by weights attached to its lower end. The arrangements for this piece were somewhat troublesome, but not such as to require a particular description. There is no cement used in its construction. The glass is attached to the stand above, and to the weights below, by double bands of thin leather, which are folded on the two ends of the slip, and kept adherent to the glass by small blocks of wood and light clamping-pieces of brass. The fixed compensator thus constructed acts very well up to a tension of sixteen pounds; and beyond this I have not yet gone.

6. *Arrangement of the Pieces.*—The optical effects obtained are sensibly modified by very small changes in the source of light, and in the positions of the pieces; but I shall not dwell upon these variations here, as they were noticed at some length in the second of my former papers. In all cases, the acting ray is horizontal, undeviated through its whole course, and about a foot distant from the table which supports all the pieces. Of the various arrangements tried, the following is, I think, the best upon the whole. The diagram shows all the pieces in horizontal section through the ray LM, L being the source of light, and M the observer's eye. L is a flat paraffin-



flame presented edgewise; P is the polarizing Nicol; A is the dielectric cell (1), having its outer balls connected by copper wires, one with the prime conductor and the other with earth; B is a couple of stationary compensating plates of glass (3), hanging vertically, and mounted so as to admit of the attachment of stretching weights to one or the other or both (5); C is a neutralizing plate which is sometimes required in measurements and in the more delicate observations; Q is the analyzing Nicol. The distance PQ is from 40 to 60 inches. The hand compensator is not shown in the diagram; when used, it is held between the pieces C and Q.

7. *Conduct of an Observation.*—The pieces are brought most conveniently into the line LM in a certain order of succession. The piece first placed is the polarizer P: it is laid carefully with its principal section at 45° to the horizon—that is, at 45° to what is afterwards the axis of the electric field. The ana-

lyzer Q is then laid so as to suit the observer's eye, and is turned into the position of perfect extinction ; and this being done, the two Nicols are left untouched, if possible, till the end of the experiment. The cell A, with conducting wires led from its outer balls to prime conductor and earth respectively, is now put in position under direction from the observer, who sits at the polariscope, and restores the light by the use of the hand compensator. When A is well placed, the line LM is perpendicular to the plate-faces of the cell, and the object restored by the hand compensator is a fine streak of light, passing midway between the balls in the cell, and projecting well above and below them. When this has been done once for all, the cell is left unmoved throughout the experiment. The fixed compensator B, containing either one plate or two, is now placed carefully, so that the line LM passes through the centre of each plate in a direction perpendicular to its faces. The last piece C is not required always, but only when the purity of initial extinction in the polariscope has been in any degree lost by the introduction of A or B ; it is a piece of common plate, the more irregular in temper the better, about $\frac{1}{8}$ of an inch thick, fixed in a movable stand ; its modes of application and action are precisely the same as those of the larger neutralizing plate described in my first paper. When things have been thus arranged, the cell is charged with clean liquid (2), the observer sits at the polariscope, and, the initial extinction being still unimpaired, the electric machine is set in motion.

Definite Chemical Compounds.

8. *Carbon Disulphide as a Dielectric.*—My first trial of the new cell was with this liquid, which is much the best dielectric yet discovered. The arrangements and procedure are exactly as described in the last two articles ; the cell is charged with perfectly clean carbon disulphide, and the initial extinction is perfect. A small movement of the machine, one turn or less, gives a very fine restoration of the light in the polariscope. As the potential rises, the light increases steadily till it is quite brilliant ; but if a spark be taken upon the knuckle from the prime conductor at any point in this process, the phenomenon vanishes instantly.

9. *Character of the Optical Effect.*—The preceding experiment is repeated, with the addition of the hand compensator (4). The compensating slip is put in position with its length horizontal, and the initial extinction is found to be unimpaired ; the light is then restored steadily by electric action, and the slip is successively stretched and compressed with forces in-

creasing continuously from zero. Horizontal tension is found to strengthen the effect of electrical action in every case, while horizontal compression, with similarly perfect distinctness and regularity, weakens the effect down to sensibly pure extinction. When the electric action is feeble, a certain small compression of the compensator extinguishes the electrically restored light as a whole, or simultaneously in all its parts; but when the action is intense, the axial part of the field requires a greater compression than the outer parts, and the extinction-phenomena take the form of two dark bands, which will be particularly described immediately.

When the plane of polarization of the light rendered by the first Nicol is either horizontal or vertical, either parallel or perpendicular to the lines of force, and the second Nicol is at pure extinction, the effects of electrization are evanescent; if they do appear, they are irregular in character and trifling in quantity.

There is certainly no rotation of the plane of polarization in the present case; for when the light restored by electric force is tried by small rotations of the second Nicol in contrary directions from the position of initial extinction, it is found to be similarly and equally affected by the two movements.

The action of dielectrified carbon disulphide upon transmitted light is therefore similar to that of *glass extended in a direction parallel to the lines of force*: it is a sensibly pure case of what is known in optics as a *uniaxally birefringent action*, the axis being *parallel* to the lines of force, and the action *positive*. Of two component vibrations, which are polarized in planes respectively *parallel* and *perpendicular* to the lines of force, the *latter* is relatively *retarded*.

10. *Carbon Disulphide as an Insulator*.—To test the insulating power of this and other liquids, I compare the striking-distance of the prime conductor when the wire connecting it with the cell is in position and out successively. When the cell is charged with carbon disulphide the result is decisive. All the pieces being placed as in the experiments just described, the machine is worked at an ordinary rate, and sparks are drawn from the prime conductor on the knuckle, or on a metallic ball connected with earth; the wire from prime conductor to cell is then removed, and sparks are again drawn from the prime conductor now simply insulated. I have seen very little perceptible difference (if any) between the two cases, the sparks being of much the same density and length when the connecting wire is in position and when it is out.

I present this fact prominently, because it has an important bearing on the interpretation of the electro-optic experiment

(8). It would be rash to assert that there is no considerable discharge of electricity through the cell in the course of that experiment. On the contrary, I think that when the wires are in position and the machine working, there is a certain quantity of convective discharge through the liquid. But the present observation shows, to a certainty, that the shell of bisulphide in the cell, though not more than one eighth of an inch thick, is able to keep the two inner balls at a large difference of potentials, a larger difference than would be supported under similar conditions by more than an inch of air. The liquid is therefore a good insulator; and the restoration of the light by electrization is due, in all probability, to electrostatic inductive action through the liquid. Up to this point I have merely given a revision of observations made long ago with the old plate cell, and published in the second of my former papers.

11. *Extinction-bands in CS₂*.—The cell is charged with clean carbon disulphide, and the pieces all stand as in the diagram of (6), the principal section of the first Nicol being inclined at 45° to the horizon, and the second Nicol being at pure extinction. The correctness of the arrangements is tested by a repetition of the electro-optic experiment with the hand compensator (9). Matters being thus arranged, I begin now by attaching a weight of some pounds, say eight or nine, to one of the fixed compensating slips B (6). The experiment begins thus with a strong permanent restoration from extinction in the polariscope, the restoration being due to vertical tension of glass, which is here optically equivalent to horizontal compression of glass, and therefore optically contrary to the electric action (9). It should be remembered that the object now restored in the polariscope is a fine vertical streak of flame-light, passing through the centre of the electric field, not encroaching on either ball, but projecting well above the balls, and also well below. Things being thus prepared, the observer sits at the polariscope, and the machine is worked at a moderate rate.

The first thing observed is a broad horizontal band, very dim and ill-defined, which crosses the flame in the axial part of the electric field. As the potential of prime conductor and inductive ball rises, the band comes out more and more definitely, and darkens by degrees till it is perfectly black, every trace of that part of the flame having disappeared. As the potential still rises the flame begins to reappear in a faint speck or patch at the centre of the band. The patch brightens and widens gradually till the one band along the axis is clearly broken up into two, lying symmetrically on opposite sides of the axis,

and concave to each other. As the potential still rises, the bands move symmetrically outwards from the axis, dividing the flame into three large segments which are sensibly of equal brightness. And when the electric action is at the strongest, near spark-discharge through the liquid, the bands cross the flame at points very little, if so much as, outside of the cylinder which envelops the two balls. The bands are still intensely black and well-defined where they cross the flame; and they are quite distinct in their whole course, as fine arches resting on the two balls, and spanning the intermediate field. When the machine is stopped, and the potential falls to zero more or less slowly, the optical effect passes through the same phases, but of course in reversed order.

12. This fine experiment presents a case of what is known in optics as the cross duplication of positive uniaxal plates, the axis of the compensating slip being vertical, and that of the liquid plate horizontal. The birefringent action of the dielectric plate is not uniform either in time or space, but increases in time at any given point of the field as the potential rises, and diminishes at each instant regularly in space, outwards from the axis of the electric field; and this consideration affords a sufficient general explanation of the form and phases of the phenomena. When the flame is divided by the bands into three segments, the light of the middle segment is restored by predominance of electric action in the liquid plate, while the light of the extreme segments is still restored by predominant action of the compensating plate of glass. I think the following variation of the experiment worth mentioning as an additional illustration of this view.

The extinction-bands being formed as in (11), and kept stationary in the outer parts of the electric field by constant motion of the machine, I introduce the hand compensator, and apply a horizontal compression beginning at zero. As the compression rises gradually to a large intensity, the arched bands move in gradually towards the axis of the field, until they coincide in one band, which finally disappears. If the compression be suddenly relieved at any point in this process, the arches come into view again at once in their old positions. Like effects are obtained by a strong downward pull upon the weight which is attached to the fixed compensator.

13. These extinction-bands, obtained by electric action against the tension of the fixed compensator, improve in all respects with every increase of tension and corresponding increase of potential. Against a tension of one pound or two, the bands are moved into the outer parts of the field by a comparatively feeble electric action, and they are wide and

dim and not well defined, although they present all the essential features of the phenomenon clearly enough ; but against a larger tension of twelve pounds to sixteen, the bands are beautifully distinct, narrow, sharply defined, and very black. Some elementary electro-optic measurements with this liquid will be described afterwards ; in the meantime I shall merely ask the reader to notice the large range of measurable optical effect here obtained, and with such a small cell, from a tension of one pound or less in the fixed compensator up to a tension of sixteen pounds.

14. *Benzol* (C_6H_6).—This liquid also had been already examined thoroughly in the old cell. When tried in the new cell it acts as a very good insulator (10), and gives excellent optical effects of the same kind as those of carbon disulphide (9). It requires no measurements, and very little observation, to show that this liquid is far inferior to the former in intensity and range of effect. Benzol does give the extinction-bands clearly ; but they are never so fine as in CS_2 ; and I think that I have never seen the bands clearly separated in benzol against a tension of more than two pounds in the fixed compensator. Still the electro-optic action is very fine, pure, and perfectly regular. The light restored from pure extinction by electric action in the cell is always extinguished perfectly by compression of glass in a direction parallel to the lines of force, and always strengthened by tension in that direction.

15. *Toluol* (C_7H_8).—This liquid is very like benzol in its more patent physical properties ; like benzol also, it is a very good insulator, and gives a good optical effect of the same kind as CS_2 under electric action. In the only two careful trials that I have given to this liquid, I found it particularly difficult to obtain a pure initial extinction ; and although this may have been caused by some unnoticed and accidental derangements of the solid parts of the apparatus, I suspect it was rather due to some faint specific action of the liquid. In other respects I could not observe any clear difference between toluol and benzol, the effects being equally regular and pure, of exactly the same kind, and of much the same intensity and range. The light restored from good extinction in the polariscope by electric action in the cell was always extinguished perfectly by horizontal compression or vertical tension of glass. The extinction-bands also were clearly developed in toluol, as in benzol, against a small tension in the fixed compensator.

16. *Xylol* (C_8H_{10}).—This liquid also is very like benzol in odour and appearance ; and it acts very similarly in the plate cell, both as an insulator and in electro-optic experiment. From

the difficulty of cleaning it, I found it not a good liquid to work with. After many rinsings of the cell, the electrified balls were still connected by visible chains of particles; and although the larger particles fell to the bottom of the cell in a little time, and the desired effect came out very clearly, still the liquid was hardly ever purely transparent, but generally somewhat misty or faintly speckled. With this one drawback, xylol acted perfectly well in the electro-optic experiment (9). The optical effect was clearly stronger than that of benzol. The extinction-bands were well developed against a tension of two pounds. The light restored by electric action was always extinguished perfectly by horizontal compression of glass.

17. *Cumol* (C_9H_{12}).—This compound is somewhat viscous, and not nearly so volatile as the preceding liquids. The only sample of it that I have worked with is of a faint yellowish colour, but purely transparent. When tested in the usual way, it acts as a good insulator (10). The optical effect of electric force is of the same kind in cumol as in the former liquids, and is equally regular and pure, being always neutralized perfectly by horizontal compression or vertical tension of glass. This liquid is likely to hold an important place in electro-optics: it is in all respects very easily managed as a dielectric; and after CS_2 , which it follows indeed at a large interval, it gives an optical effect more intense and of longer range than any other liquid yet examined. Against a tension of four pounds in the fixed compensator, the extinction-bands are developed almost as finely in cumol as in CS_2 .

When the electric action and the compensating strain are intense, the bands appear to assume a peculiar form in cumol. Returning to the experiment described in (12), where the two compensators were applied in combination with an intense electric action on CS_2 , it will be remembered that the effect of a strong compression of the hand compensator was to bring the bands in towards the axis of the field, where they finally coincided in one axial band. In cumol the effect takes a different form. As the bands approach the axis, they become largely inclined to each other, converging from the outer parts of the surface of the inductive ball towards the intersection of the axis of the field with the surface of the opposite ball. A contrary form of effect was observed in xylol, where the bands diverged from the axial part of the surface of the inductive ball towards the outer parts of the opposite ball. Other liquids gave traces of similar variations, but none so distinctly as the two that I have mentioned. Although I have not made a particular study of these phenomena, but have merely noticed them carefully in passing, I cannot believe

them to have been accidental. They seem rather to indicate some specific differences between the several liquids, probably with reference to the distribution of electric force.

18. *Cymol* ($C_{10}H_{14}$).—Colourless, transparent, and very distinguishable from benzol by its agreeable odour. It is an excellent insulator, and acts very similarly to benzol in electro-optic experiments, giving an optical effect of exactly the same kind, equally regular and pure, and of much the same intensity and range. The light restored by electric force in cymol is always extinguished perfectly by compression of glass in a direction parallel to the lines of force.

19. *Terebene* ($C_{10}H_{16}$).—When the cell is charged with my only specimen of this liquid, the arrangements being otherwise as in the fundamental electro-optic experiment (8, 9), I find, contrary to expectation, that the light is rotated and sensibly dispersed in its passage through the cell; still there is a good approximate initial extinction got between the red and blue. The liquid acts as a very good insulator, the sparks from prime conductor having apparently the same density and length when the connecting wires are in and out of place (10). In terebene, electric force evidently strengthens the light from approximate extinction in the polariscope; and this effect is clearly weakened by horizontal compression of glass, and clearly strengthened by horizontal tension. The effect is not nearly so strong as in the members of the benzol series; but it is certain, and certainly of the same kind. My former observations on oil-of-turpentine with the old plate cell were at least as satisfactory as these on terebene.

20. *Amylene* (C_5H_{10}).—Colourless, transparent, and excessively volatile. Tested in the usual way, it acts as an excellent insulator; long sparks from the prime conductor, or from connected ball of the cell, do not sensibly diminish in length or intensity when the earth-ball of the cell is touched by earth-wire or knuckle. In the electro-optic experiment, as for CS_2 (8, 9), amylene acts very finely, and in the same way as each of the preceding hydrocarbons, the effect of electric action from extinction in the polariscope being always neutralized perfectly by horizontal compression of glass. Against a weight of one pound on the fixed compensator (11), electrization develops the extinction-bands faintly, and moves them into the outer parts of the field; but against a weight of two pounds, the effect of the strongest electric action attainable falls far short of extinction at the centre of the electric field. Amylene stands, therefore, between benzol and terebene.

21. *Valeric Acid* ($C_5H_{10}O_2$).—Tested in the usual way, this liquid is not a good insulator. Sparks from the prime

conductor are very much shortened and attenuated when the connecting-wires are placed ; and if the earth-wire be detached from the cell, the earth-ball of the cell (with shaft projecting a little way out of it) gives crackling discharge into the air whenever the machine is worked vigorously. In one careful trial of the electro-optic experiment, as for CS_2 , I obtained a continuous restoration from pure extinction in the polariscope. The effect was extremely faint, but perfectly regular, and was neutralized by horizontal compression of glass. In several following trials I could not recover this phenomenon regularly. I have little doubt that the effect is real ; but it is one of the faintest that I have ever observed.

22. *Carbon Dichloride* (C_2Cl_4).—Transparent, colourless, and a very good insulator. Under electric action, as in the preceding experiments (8, 9), carbon dichloride restores the light from extinction in the polariscope. The effect is pure and regular, and is neutralized perfectly by horizontal compression of glass. The extinction-bands are well separated in this liquid against a weight of three pounds on the first compensator (11). Carbon dichloride stands, therefore, somewhat above benzol.

As this liquid was expensive, and as there was only a small quantity of it at hand, I had to be satisfied with charges that were not quite clean. As the experiment proceeded, the impurities were apparently dissolved or absorbed in some way, and the plate improved remarkably in its optical action. Changes of the same kind were observed in some other liquids, but in a less degree.

23. At this point I may mention some apparently trivial phenomena which I observed repeatedly in the course of the preceding experiments. On one occasion, when the cell was charged with CS_2 , the liquid had been allowed to evaporate until its free surface was nearly as far down as the tops of the balls. At the instant when the machine was set in motion, the surface of the liquid was deformed ; over the centre of the field there was a hump raised which, by its form and position, reminded me of the arched extinction-bands (11). As long as the electric force was kept at a moderate and approximately constant intensity, I could not detect any sure appearance of motion in the hump. Similar effects, though not so large, were obtained in benzol, also in carbon dichloride and other liquids. In cumol the hump was apparently as high as in CS_2 ; but it was accompanied by vigorous movements in the liquid, apparent currents from ball to ball along and through the hump. In xylol the effects were very intense ; the movements were more violent than in cumol ; and when only a large bubble of

air was left in the top of the cell, it was drawn downwards by the commotion of the strongly electrified liquid, and was broken up into many small bubbles, which danced rapidly through the field, and prevented all regular optical effect.

The impression conveyed by the phenomenon in the case of CS_2 was that of a simply statical arrangement of the dielectric, a concatenation of electrically polarized particles of the liquid along the curved lines of force, the electric action being intense enough to overpower, so far, the gravitation of the particles. In the case of cumol the impression was different; the hump may have been produced, at least in part, by the strong convection-currents which always accompanied it. Not that there is any real inconsistency between these views; for there may evidently be a regular file-arrangement of particles, kept up continually, or, rather, incessantly renewed, along the curved lines of force, while there are gross currents of liquid passing incessantly between the balls.

24. *Nitrobenzol* ($\text{C}_6\text{H}_5\text{NO}_2$), *new electro-optic action*.—This oily liquid, though yellowish in colour, is very purely transparent. Tested in the usual way, it acts as a good conductor (10). When the two connecting-wires are in position, and the machine is worked vigorously, no sensible spark can be drawn from the prime conductor, no movement can be detected in the liquid, nor any trace of the hump (23). And, accordingly, in the electro-optic experiment arranged and conducted as for CS_2 (8), nitrobenzol gives no trace of optical effect, the extinction in the polariscope being as pure when the machine is worked at the hardest rate as when it is at rest. But it requires only a small change of the conditions to give a large effect.

The first connecting-wire, that from prime conductor to cell, being kept always in position, the earth-wire is detached from the second outer ball of the cell, and the observer at the polariscope brings up his knuckle slowly towards the latter ball till a spark passes, the longer and denser the better. At the instant of the spark (that is, at the instant of abrupt discharge of the prime conductor through the liquid) there is a strong restoration from extinction in the polariscope, not a mere spark, nor a vague illumination, but a true restoration of the old object, bright and clear and well outlined, as in many of the former observations, though apparently instantaneous as the spark itself.

As the knuckle is brought up slowly to contact with the ball, the restorations in the polariscope succeed each other more rapidly, and become individually fainter. The light is by-and-by sustained continually, but never without a sen-

sible flicker ; and it becomes fainter and fainter on the whole as the length of the spark diminishes. A little before the spark vanishes, the restorations disappear to sense ; and from this point up to contact of ball and knuckle, and afterwards, the action of the machine is without sensible effect in the polariscope. I have made some additional observations on this form of effect ; but, to prevent confusion, I reserve them for a little.

25. *Bromtoluol* (C_7H_7Br).—In its electro-optic relations, this liquid resembles carbon disulphide and benzol on the one hand, and nitrobenzol on the other, giving the two kinds of effect clearly, though not intensely. Tested in the usual way, it acts as an imperfect insulator. When the connecting-wires are in position, the sparks from the prime conductor are greatly attenuated, and are reduced in length from an inch or more to about an eighth of an inch.

When bromtoluol is examined electro-optically in the same way as CS_2 (8), the electric action gives a continuous restoration from pure extinction in the polariscope. The effect is not strong ; but it is quite certain, and certainly of the same kind as in CS_2 , being neutralized perfectly by horizontal compression of glass. And, again, when the discharging train from the prime conductor through cell to earth is interrupted at any point by an air-interval, there is a good restoration in the polariscope at the instant of each spark, the effect being apparently of the same kind as that observed in nitrobenzol, though not nearly so strong. Bromtoluol is an inconvenient liquid to work with, partly from the very irritating, onion-like action of its vapour on the observer's eye, and partly from the difficulty of getting a clean charge—a difficulty that I have not once overcome perfectly.

26. *Trial of other Liquids for the Nitrobenzol Effect*.—Carbon disulphide, carbon dichloride, terebene, and most of the other liquids already mentioned were all examined carefully in the same way as nitrobenzol (24). The optical effects accompanying discharge were either insensible, or of the kind noted in the two following cases.

Amylene.—The sparks obtained from the earth-ball were very short and attenuated, and were accompanied by distinct restorations in the polariscope. The restorations were far from instantaneous, each of them rising sensibly to a maximum intensity, and then sensibly falling. The effect improved in strength as the knuckle approached the ball till the length of the spark reached zero ; and then the effect was to sense continuous, and certainly at its greatest intensity.

Benzol.—The sparks from earth-knob to knuckle were very

short, not more than an eighth of an inch, when those from the prime conductor were about an inch. The sparks were accompanied by an almost continuous effect in the polariscope, which improved as the knuckle approached the ball. At contact of ball and knuckle the optical effect was continuous, and certainly at its greatest intensity. In these two liquids, and in all the other insulators that gave any distinct effect, the phenomenon was apparently of the same kind through its whole progress, and very unlike that obtained from nitrobenzol.

27. *Stannic Chloride* (SnCl_4).—I was sorry to find, on trial, that my method is quite inapplicable in the case of this interesting compound, one of the best insulators known among liquids. Still the trial ought to be described, as it gave me a good glimpse of what I believe to be a new fact. The liquid was let into the cell as rapidly as possible, through filtering-paper and funnel. During this brief exposure to the air, which was unfortunately rather moist at the time, the liquid gave off a dense white cloud of suffocating fumes. The liquid, as it rose in the cell, was fairly transparent; but above it there lay a deep shell of dense white froth. The charging of the cell was completed in several seconds. The most of the froth had run over the mouth of the vertical boring, and was rapidly wiped away; but some of it had evidently dissolved in the liquid, giving it a uniformly milky or misty appearance, deadening the transparency without seriously impairing it. As all the pieces had been already put in final position for the electro-optic experiment (8, 9), the second Nicol was now turned at once to good extinction, and the machine was set in motion. At the first turn of the plate there was a vivid restoration in the polariscope, stronger a great deal (such at least was my impression at the time) than any thing of the kind that I had yet seen, even in the case of CS_2 . The hand compensator was immediately applied in the usual way (4), but without a trace of the ordinary effect: the light restored by electric force was not sensibly weakened either by horizontal tension or by horizontal compression. This result was so unexpected, and, indeed, so extraordinary, that I had to spend a little time in making sure there was no mistake. Trying other means, I soon found that the effect of electric force was neutralized, either perfectly or very nearly, by a definite rotation of the second Nicol through a small angle; and up to this point I am very confident of the facts.

By this time the liquid had evidently deteriorated, not as an insulator, but as an optical medium; it had become in appearance faintly discontinuous, partly speckled and partly reticu-

lated, showing that the fine deposit was in course of agglomeration. Among other trials now made, the first Nicol was turned through a right angle, the second Nicol was turned into the position of best attainable extinction, and the electro-optic observation was repeated. Nothing like a good extinction was obtained in this case; but the effect of electric action was still considerable; and it was evidently and considerably weakened by a rotation of the second Nicol through a small angle, the direction of this rotation being contrary to that obtained in the first case. Several other charges of the cell were tried from the same bottle; but they were all speckled or gritty from the outset, and gave no distinct effect. As far as I can judge from memory and from imperfect notes taken at the time, I think that the optical action of electric force thus manifested in stannic chloride is to diminish the acute angle between the plane of polarization and the lines of force.

Such is the best account that I can give of an observation which was unavoidably hurried, confused, and unsatisfactory. The experiment is not one that I should like to repeat in the same form; it puts the instruments quite out of working order, and tends also to damage them permanently.

28. *Other Liquids tried, but without effect.*—Of these I may mention particularly chloride of sulphur, pentachloride of antimony, trichloride of phosphorus, tetrachloride of carbon, sulphide of allyl. These all acted as conductors (10), and gave no sensible effect in the polariscope under electric force (8). I should except sulphide of allyl, which gave good, though very faint, traces of the nitrobenzol effect (24). I have little doubt that the failure of most of the perchlorides was due to traces of water, for which these compounds have an intense attraction.

All the liquids mentioned up to this point were obtained as pure chemicals from the establishment of Burbidge and Farries.

Organic Liquids.

29. *Young's Paraffin Oil.*—Specific gravity $\cdot 814$, a trade sample of an illuminant, one of the lightest made, clear as water, and an excellent insulator. In the electro-optic experiment, as for CS_2 , this liquid gives a very fine, but faint effect: the light is well restored by electric force from pure extinction; and the effect is neutralized perfectly by horizontal compression of glass. In intensity and range of effect, this paraffin stands between amylene and terebene, but a good deal nearer the latter. An illuminating paraffin was tried long ago in the old plate cell, and with like effects, though much fainter.

30. *Young's Paraffin Oil*, specific gravity $\cdot 890$, a trade sample of one of the heaviest lubricants made; brownish in colour, fluorescent, transparent, and a very good insulator. In electro-optic experiments the action of this paraffin is similar to that of the preceding, equally regular and pure, but remarkably stronger. The extinction-bands are as fine in this liquid as in cumol, if not finer. Against a weight of 4 pounds on the fixed compensator the bands are easily moved out beyond the balls; but against a weight of 7 pounds the axial band is barely, if so much as, divided. There is no other liquid that I have examined, except CS_2 , which is clearly superior to this heavy paraffin in strength and range of electro-optic action. I am not sure whether cumol should be placed above or below it.

Glasgow, July 1, 1879.

[To be continued.]

XII. *On Professors Ayrton and Perry's new Theory of the Earth's Magnetism, with a Note on a new Theory of the Aurora.* By H. A. ROWLAND, *Professor of Physics in the Johns Hopkins University*.*

SOME years ago, while in Berlin, I proved by direct experiment that electric convection produced magnetic action; and I then suggested to Professor Helmholtz that a theory of the earth's magnetism might be based upon the experiment. But upon calculating the potential of the earth required to produce the effect, I found that it was entirely too great to exist without producing violent perturbations in the planetary movements, and other violent actions.

I have lately read Professor Ayrton and Perry's publication of the same theory; and as they seem to have arrived at a result for the potential much less than I did, I have thought it worth while to publish my reasons for the rejection of the theory.

The first objection to the theory that struck me was, that not only the relative motion but also the absolute motion through space of the earth around the sun might also produce action. And to this end I instituted an experiment as soon as I came home from Berlin.

I made a condenser of two parallel plates with a magnetic needle enclosed in a minute metal box between them; for I reasoned that, when the plates were charged and were moved forward by the motion of the earth around the sun, they would then act in opposite directions on the enclosed needle, and so

* Communicated by the Physical Society, having been read June 29th.

cause a deflection when the electrification of the condenser was reversed. On trying the experiment in the most careful manner, there was not the slightest trace of action after all sources of error had been eliminated.

But the experiment did not satisfy me, as I saw there was some electricity on the metal case surrounding the needle. And so I attacked the problem analytically, and arrived at the curious result that if an electrified system moves forward without rotation through space, the magnetic force at any point is dependent on the electrical force at that same point—or, in other words, that all the equipotential surfaces have the same magnetic action. Hence, when we shield a needle from electrostatic action, we also shield it from magnetic action.

This theorem only applies to irrotational motion, and assumes that the elementary law for the magnetic action of electric convection is the same as the *most simple* elementary law for closed circuits. Hence we see that, provided the earth were uniformly electrified on the exterior of the atmosphere, there would be no magnetic action on the earth's surface due to mere motion of translation through space.

In calculating the magnetic action due to the rotation, I have taken the most favourable case, and so have assumed the earth to be a sphere of magnetic material of great permeability, μ . It does not seem probable that it would make much difference whether the inside sphere rotated or was stationary; or at least the magnetic action would be greatest in the latter case; and hence by considering it stationary we should get the superior limit to the amount of magnetism.

Let a be the radius of the sphere moving with angular velocity w , and let σ be its surface-density in electrostatic measure, and n the ratio of the electromagnetic to the electrostatic unit of electricity. Then the current-function will be

$$\phi = \frac{\sigma}{n} wa^2 \int \sin \theta d\theta = -\frac{\sigma}{n} wa^2 \cos \theta.$$

Hence (Maxwell's 'Treatise,' § 672) the magnetic potential inside the sphere is

$$\Omega = -\frac{8\pi}{3} \frac{\sigma}{n} war \cos \theta,$$

and outside the sphere

$$\Omega' = \frac{4}{3} \pi \frac{\sigma}{n} wa^4 \frac{\cos \theta}{r^2}.$$

The magnetic force in the interior of the sphere is thus

$$F = \frac{8}{3} \pi \frac{\sigma}{n} wa,$$

or the field is uniform. If the electric potential of the sphere on the electrostatic system is V , we may write

$$F = \frac{2}{3} \frac{w}{n} V,$$

which is independent of the dimensions of the sphere.

In this uniform field in the interior of the sphere, let a smaller sphere of radius a' be situated; the potential of its induced magnetization will be

$$\Omega_1 = \frac{\mu-1}{\mu+2} a'^3 F \frac{\cos \theta}{r^2}.$$

Hence the expression for the potential for the space between the two spheres will be

$$U = \frac{2}{3} \frac{w}{n} V \left\{ -r \cos \theta + \frac{\mu-1}{\mu+2} a'^3 \frac{\cos \theta}{r^2} \right\};$$

and outside the electrified sphere it will be

$$U' = \frac{w}{n} V \left(a^3 + 2a'^3 \frac{\mu-1}{\mu+2} \right) \frac{\cos \theta}{r^2}.$$

Let us now take the most favourable case for the production of magnetism that we can conceive, making $a' = a$ and $\mu = \infty$; we then have

$$U' = \frac{w}{n} V a^3 \frac{\cos \theta}{r^2},$$

which is the potential of an elementary magnet of magnetic moment

$$\frac{w}{n} V a^3.$$

But Gauss* has estimated the magnetic moment of the earth to be

$$3.3092 a^3$$

on the millimetre mg. second system. Hence we have

$$V = 3.3092 \frac{n}{w}$$

for the potential in electrostatic units on the mm. mg. second system. In electromagnetic units it is thus

$$V_1 = 3.3092 \frac{n^2}{w};$$

and hence in volts it is this quantity divided by 10^{11} .

* Taylor's Scient. Mem. vol. ii. p. 225.

As the earth makes one revolution in $23^h 56' 4''$, or in 86164 seconds, we have

$$w = \frac{2\pi}{86164};$$

and

$$n = 299,000,000,000^* \text{ millims. per second.}$$

Hence the earth must be electrified to a potential of about

$$41 \times 10^{15} \text{ volts}^\dagger$$

in order, under the most favourable circumstances, to account for the earth's magnetism. This would be sufficient to produce a spark in atmospheric air of ordinary density of about

$$6,000,000 \text{ miles!}$$

Professors Ayrton and Perry have only found the potential 10^8 volts, or 400,000,000 times less than I find it.

It was this large quantity which caused me to reject the theory; for I saw what an immense effect it would have in planetary perturbations; and I even imagined to myself the atmosphere flying away, and the lighter bodies on the earth carried away into space by the repulsion. And, doubtless, had not Professors Ayrton and Perry made some mistake in their calculation by which the force was diminished 16×10^{16} times, they would have feared like results.

For according to Thomson's formula, the force would be equal to a pressure outwards of

$$r = \frac{V^2}{8\pi a^2},$$

which amounts to no less than

$$1,800,000 \text{ grms.}$$

per square centimetre! or 10,000 kil. per square inch! Such an electrostatic force as this would undoubtedly tear the earth to pieces, and distribute its fragments to the uttermost parts of the universe. If the moon were electrified to a like

* From a preliminary calculation of a new determination made with the greatest care, and having a probable error of 1 in 1300.

† That this is not too great may be estimated from my Berlin experiment, where a disk moving 5,000,000 times as fast as the earth with a potential of 10,000 volts, produced a magnetic force of $\frac{1}{50000}$ of the earth's magnetism,

$$5,000,000 \times 10,000 \times 50,000 = 2,500,000,000,000,000,$$

which is of the same order of magnitude as the quantity calculated, namely 61×10^{15} . It can be seen that this reasoning is correct, because the formulæ show that two spheres of unequal size, rotating with equal angular velocity and charged to the same potential, produce the same magnetic force at similar points in the two systems.

potential, the force of repulsion would be greater than the gravitation attraction to the earth, and it would fly off through space.

For these reasons I rejected the theory, and now believe that the magnetism of the earth still remains, as before, one of the great mysteries of the universe, toward the solution of which we have not yet made the most distant approach.

In connexion with the theory of the earth's magnetism, I had also framed a theory of the Aurora which may still hold. It is that the earth is electrified, and naturally that the electricity resides for the most part on the exterior of the atmosphere—and that the air-currents thus carry the electricity toward the poles, where the air descending leaves it—and that the condensation so produced is finally relieved by discharge.

The total effect would thus be to cause a difference of potential between the earth and the upper regions of the air both at the poles and the equator. At the poles the discharge of the aurora takes place in the dry atmosphere. At the equator the electrostatic attraction of the earth for the upper atmospheric layers causes the atmosphere to be in unstable equilibrium. At some spot of least resistance the upper atmosphere rushes toward the earth, moisture is condensed, and a conductor thus formed on which electricity can collect; and so the whole forms a conducting system by which the electric potential of the upper air and the earth become more nearly equal. This is the phenomenon known as the thunder-storm.

Hence, were the earth electrified, the electricity would be carried to the higher latitudes by convection, would there discharge to the earth as an aurora, and passing back to the equator would get to the upper regions as a lightning discharge, once more to go on its unending cycle. I leave the details of this theory to the future.

Baltimore, May 30, 1879.

Appendix.—Since writing the above, Professors Ayrton and Perry's paper has appeared in full; and I am thus able to point out their error more exactly. Their formula at the foot of page 406 is almost the same as mine; but on page 407, in the fourth equation, the exponent of n should be $+\frac{1}{2}$ instead of $-\frac{1}{2}$, which increases their result by about 600,000,000, and makes it practically the same as my own.

Rotterdam, July 13th.

XIII. *Carbon and Carbo-Hydrogen, Spectroscopied and Spectrometed in 1879.* By PIAZZI SMYTH, *Astronomer Royal for Scotland**.

THE recent spectroscopic observations of Brorsen's Comet having brought again into view the strange contradiction which still exists, between chemists on one side, and most spectroscopists on the other, touching the temperature at which pure carbon vapour can possibly be volatilized, the present may be an appropriate occasion for describing a few recent steps in the inquiry, especially as they may haply be found to shed some new light on the difficulty undoubtedly inherent in the case.

On the one hand, the chemists are represented as uniformly agreed that pure carbon is, to all their methods of trial by furnace-heat, next to impossible to drive off into vapour. And, on the other hand, not only some, but I am told that almost all the great spectroscopists of the day have found, according to their examinations of precision, that the vapour of carbon is given off freely in the moderate heat of every kind of little candle- or lamp-flame (!)—this being evidenced to them by a spectrum which they declare is that of pure carbon, though some two or three persons in, by this time perhaps rather to be considered out of, the community, persist in holding it to be the spectrum of only the compound gas “carbo-hydrogen.”

This last spectrum, carefully described by Professor Swan, in Edinburgh in 1856, as being that of “carbo-hydrogen,” was nine years later pronounced by Dr. Attfeld in London to belong to pure “carbon,” and has now, after many intervening researches by other men, very recently been still more minutely examined by me in its most standard form of a coal-gas and common-air blowpipe-flame, viewed *end on*. This mode of observing it is of extraordinary advantage for seeing the fainter lines, and allowing higher dispersive power of prisms to be employed. But it did not actually reverse any thing of moment in Professor Swan's primary description. It only extended and added details—utilizing even most of the mere haze which earlier observers had noticed hanging about the bands of graduated lines, by resolving it into little attendant lines or linelets. Hence there is no dispute about all the chief physiognomy of this spectrum; and the only difference of opinion is, the rather extreme case in physics, as to whether it belongs to the easily volatile compound “carbo-hydrogen,” or the most refractory and involatile element “carbon.”

* Communicated by the Author.

The startling deduction that the spectrum belongs to carbon is claimed to have been proved by Dr. Attfield, Dr. Marshall Watts, and other most able men, from their having found one and the same spectrum in all compounds of carbon—*i. e.* not only in carbo-hydrogens, but in carbo-oxygens and carbo-nitrogens of several kinds, first in blowpipe flames, and then in gases illumined by the electric spark, both at ordinary atmospheric pressure and in vacuum-tubes.

Of these several methods I have only been able to test the last through many varieties of carbon, and other, compounds. But that method I have not only had superior vision of by means of my recently constructed end-on tubes (made for me by M. Salleron, 24 Rue Pavée au Marais, Paris), but have examined its manifestations under more powerful dispersion than most of my predecessors. And with what result? With the astonishment of finding that after all, while something certainly visible is undoubtedly common to all the tubes, there is another thing visible, in some of them even to greater brightness, and is so seen in carbo-hydrogen tubes only.

First, let us be quite clear on what that previous something was, which was common to all the tubes.

It is a spectrum somewhat like the coal-gas and air blowpipe-flame spectrum, having, besides other features, five coloured bands—red, citron, green, blue, and violet—and each band capable of breaking up into thin compound lines under high dispersion. But no band of one of these spectra begins exactly in the same place as any band in the other; nor are its minuter constructional lines in the least degree similar. Dr. Watts, moreover, does recognize this vacuum-tube spectrum as being different from the coal-gas blowpipe-flame's spectrum, and calls it therefore "the *second* spectrum of carbon." A carbon-spectrum, too, I will not deny it may be; for the temperature of the disruptive induction-spark under which it is produced, may be quite enough to dissociate even that element from its compounds and keep its vapour incandescent. But let us examine the said possible carbon-spectrum from tube to tube, and note the variations.

Now here our first entry must be, that this alleged tube carbon-spectrum appears in *every* vacuum-tube I possess, whether purporting to contain a compound of carbon or not; but it varies in brightness. Hence it appears *moderately* only in tubes of

Air,
Nitrogen,
Oxygen,

Ozone,
Hydrogen, and
Nitrous Oxide;

but it manifests itself excessively in tubes of

Cyanogen (carbo-nitrogen).

Carbonic acid and carbonic oxide (carbo-oxygen).

Also with extreme brightness in tubes of

Alcohol,

Olefiant gas, and

Marsh-gas (all carbo-hydrogens).

Whence we may safely conclude that the spectrum appears in the six first tubes solely as an impurity of some kind ; and the abundant carbonate of soda used by the glass-makers for such tube-glass may be its origin. Hence it is the excess of intensity between those six first, and the six last, tubes which we are to look to alone as the possible carbon-spectrum of the carbonic compound gas proper to the tube. Pure hydrogen-lines (probably derived from infinitesimal traces of moisture) also appear in every one of the 12 tubes, except the cyanogen ; and there they are strikingly absent.

Confining our attention now, for the sake of greater accuracy with economy of time and labour, when using a high dispersion (33° from A to H, and mag. power = 10), to the green-band region alone of the tube-carbon-spectrum, that band does seem to be, throughout all the tubes, identical—even microscopically identical in every one of its *earlier* and *brighter* minute constructional lines ; and it varies only, at least in that earlier part, in the mere matter of general development or strength and intensity from tube to tube.

Those constructional lines of the vacuum-tubes' green band are closest and brightest on that side of it which is toward the direction of least refrangibility (*i. e.* towards the red), beginning at Wave-Number place 48,861 ; and they include, after a few of their lines towards the direction of greater refrangibility (*i. e.* towards the violet), or at Wave-Number place 48,969, a peculiar crossing over each other of two sets of lines. That spot, marked extra brightly by one line just overlapping another, is very easily identifiable by the eye, at least in my spectroscope, even when the spectrum is but faintly developed ; and it forms a convenient step for an observer, when first roughly journeying optically from the strong and sharp beginning or least refrangible and the *red-ward* edge of the said vacuum-tube carbon's green band, onwards to its more refrangible and fainter, and *violet-ward* regions, in search of any thing possibly abnormal in any one tube or another.

Instituting this quest, if we examine all our first mentioned

six tubes, viz. air, nitrogen, oxygen, ozone, hydrogen, and nitrous oxide, we shall find little or nothing unusual therein, but only see the fainter and still more faint prolongations of the green band going off in that violet-ward direction.

In cyanogen, carbonic acid, and carbonic-oxide tubes, however, there is something suspicious that in one certain spot in each of them—as far beyond, or violet-ward, of the crossing-place of lines as that is from the strong red-ward beginning of the tube-carbon's green band, there is a sort of smear, or half rubbed-out line—in fact, looking more like an impurity trace than a proper constituent of the chemical compound under trial.

But in the alcohol and olefiant gas-tubes (and even in the marsh-gas as well, though not so strongly) that sort of chalked place is occupied by a brilliant colossal bright line, so glaring that it almost extinguishes by its superior light the fine thin lines of the tube-carbon band in the neighbourhood. And twice as far onward, still in the direction of increased refrangibility (or violet-ward), there is another similar and, though less bright line, still a very notable one to meet with in that part of the spectrum.

Now what are these two bright lines which appear so conspicuously in all three of our carbo-hydrogen tubes, but in no other compounds of carbon, though we have been hitherto told by several famous observers that there is no difference amongst any of them?

By most careful reference from the electrically illuminated vacuum-tubes to the blowpipe flame, I ascertained that the strangers were the first and second lines of the green band of the coal-gas and air blowpipe-flame. Proving that even at induction-spark temperature that remarkable and humanly most useful compound, carbo-hydrogen, excitable at first by merely the flame of the smallest candle, is not yet completely dissociated into its elements.

Perhaps more powerful sparks than the one-inch ones employed by me might dissociate the whole of the carbo-hydrogen in the tube; and then we should have only the tube-carbon bands and hydrogen-lines with some possibility of impurities, while the poor old blowpipe-flame's spectrum would be nowhere. And whether some immensely and still more powerful and also jar-condensed electric sparks may not be able yet further even to break down the already described tube-carbon spectrum with its bands (composed really of innumerable and closely-packed, but very thin, sharp lines and linelets), and produce a second and linear spectrum of carbon composed probably of a few only, and far brighter and diffe-

rently placed lines, may well admit of hope, and persuade to the trial those who have the requisite apparatus. So that they may thus yet be able to do for carbon, what the late lamented Professor Plücker asserts he did for nitrogen, when by dint of 6-inch sparks and Leyden-jar condensers he changed the form of nitrogen's spectrum from its earlier, or cooler, condition of numerous bands composed of innumerable ranks of microscopic linelets, into its now well-known (and first by M. Ångström discovered) linear character of a few much brighter lines only*.

Spectromety.

Premising then once more, that though by means of the surpassing brilliancy of *end-on* vacuum-tubes and large dispersion, I may be able to give many observed and sharp lines in the green band of tube-carbon (more perhaps than have ever been previously registered for it within the same narrow limits of spectrum place), they form part of only the possible *band-spectrum* of carbon, our vacuum-tube-carbon, after all—I now propose to append a table of measures to show exactly where the first and second big lines, as well as the numerous little linelets of the compound carbo-hydrogen's green band so signally come in as an addition, when a carbo-hydrogen, and not a carbo-oxygen or carbo-nitrogen, tube is employed.

Now the first W.N. column in our duplex Table shows the tube-carbon spectrum's green band, as it was seen in a *carbonic-oxide* (carbo-oxygen) vacuum-tube. And if we simply add to that spectrum what is given in the second W.N. column, or the blowpipe-flame spectrum's green band, we shall have the tube-carbon green band very nearly as it presents itself in a *carbo-hydrogen* vacuum-tube. Very nearly, I say only, not quite; for the actual spectrum observed in the said tube gives the ribbing of the linelets of carbo-hydrogen (between say 49,400 and 49,800 W.N. place) clearer and on a darker background than what would result by artificially combining or superimposing on each other the separately observed carbonic-oxide tube-carbon spectrum and the blowpipe-flame's carbo-hydrogen spectrum.

Then in that case carbonic-oxide has added certain weak hazy bands to the fainter and more violet-ward parts of the

* While copying out this paper for the press I have heard from Prof. Alex. S. Herschel of a memoir by M. Thalén, the admirable spectroscopist of Upsala, who seems to have already obtained, in concert then with his now deceased friend M. Ångström, just such a linear spectrum of carbon. But exactly how, I have yet to learn, as the memoir has not yet arrived in Edinburgh.

pure tube-carbon-spectrum, just as carbo-hydrogen adds its brilliant lines and many linelets when it has the opportunity in one of its own tubes.

Exactly so ! But in such case what does a carbo-nitrogen tube do ?

On examining carefully I found that it added a feature excessively faint, but perfectly peculiar to itself, and somewhat reminding one, though at extreme distance, of the bands of nitrogen; for there were many rather regular haze-bands, beginning sharply, sometimes with an actual line, on the side of least refrangibility. One such line I would particularly call attention to, of intensity 1.0, and at W.N. place 49,543; for while it appeared so very clearly in a cyanogen-tube, I convinced myself again and again that it did not exist (unless homœopathically as an impurity trace) in either carbo-hydrogen or carbo-oxygen tubes.

Here, then, we have arrived at a most notable stage in our general inquiry; for by pushing the examination to further exactitude than has been usual, we have found, in direct opposition to general belief hitherto :—

1st. That each of the three varieties of compound carbon gases, viz. carbo-hydrogen, carbo-oxygen, and carbo-nitrogen, gives the later, or more violet-ward, details of the tube-carbon's band-spectrum (under 1 inch induction-spark) differently from the other.

2nd. That the feature which is thus peculiar in a carbo-hydrogen tube is undoubtedly the well-known and most brilliant coal-gas and air blowpipe-flame's carbo-hydrogen spectrum.

3rd. Whence we conclude that the pale and weak residual feature which is peculiar to a carbo-oxygen tube must therefore be the carbo-oxygen flame's spectrum in open air, and the feature peculiar to the carbo-nitrogen tube the carbo-nitrogen flame's spectrum similarly.

4th. In which case, attending to what has been already remarked as to respective intensity and faintness of lines and haze in the observations, we may see that whatever carbo-hydrogen succeeds in introducing into one of its own tube-spectra is just as remarkable for enormous overpowering force as what either the carbo-oxygen or carbo-nitrogen introduces is for ultra weakness.

5th. Wherefore in flame-spectroscopy, when burning carbo-hydrogen gas in a blowpipe, the carbo-oxygen and carbo-nitrogen impurities, even though present to a large percentage, cannot make their faint spectra appear in the presence, or to the prejudice, of the carbo-hydrogen ; while, again, if we take

primarily either of those (carbo-oxygen or carbo-nitrogen) compound gases and burn it in a blowpipe, then if the smallest trace of carbo-hydrogen, merely as an unavoidable impurity, be present, its spectrum will overpower that of the gas proper to the occasion, and may lead to some unfortunate generalizations.

POSTSCRIPT.—By the kindness of Prof. Alexander S. Herschel I am now enabled to add the veritable *linear* spectrum of carbon as given by M. Thalén in *Nova Acta R. S. Sc. Upsala*, Series iii., vol. ix. 1875, in both description, number, and graphical representation.

It was obtained apparently by the disruptive discharge of electricity of high tension and in large quantity (“*décharge disruptive d’un grand condensateur, bobine de Ruhmkorff grande dimension*”), and records only eleven lines in the whole spectrum; but each of them is remarkable for strength and clearness, thus :—

Colour.	Subject.	Intensity 1 to 10.	Appear- ance.	Wave- Number Place per Brit. inch.
Scarlet	Grand double { 1st component	8	} {	38,584
	line..... { 2nd „	9		38,616
Yellow	Single line	5		44,607
Yellow and Citron ...	Triple group... { 1st component	5	} {	44,869
		6		44,983
		4		45,046
Green	Single line	3		47,220
Green	Triple group... { 1st component	5	} {	49,315
		6		49,376
		4		49,483
VIOLET	Very broad line	10		59,540

Hence these are the *lines* which should be alluded to, with all the responsibility of their fearfully high temperature of production, whenever any one in future speaks of “carbon lines.”

Indeed M. Thalén goes further, and declares that this is the one, and only, spectrum that *carbon* alone is capable of under any circumstances whatever! But as he allows that his demonstration of that point is not yet quite complete, and as he does not seem to have discovered the peculiar microscopic arrangement of the linelets in what I have ventured to call the vacuum-tube carbon’s *band*, or lower-temperature, spectrum, I conclude here with giving my recent measures of them in their green band; thus—

Spectrometric Observations, with Prism Train of 33° Dispersion, between A and H (Solar), and mag. power of Telescope = 10. Edinburgh, May 1879.

The Green Band of Carbon, beginning with its least-refracted or redward side: as seen in an end-on Carbonic-oxide vacuum-tube, illuminated by 1-inch sparks from a small induction-coil, and a little bichromate battery of 5 pots, each exposing 10 square inches of zinc.					The Green Band of Carbo-hydrogen, beginning with its least refracted red-ward side: as seen in a coal-gas- and air blowpipe-flame, viewed end-on.				
Object observed. (Bright lines or haze.)	Intensity, ap- prox- imately.	Appear- ance, approx- imately.	WAVE- NUMBER Spectrum Place per Brit. inch.	Differences.	Object observed. (Bright lines or haze.)	Intensity, ap- prox- imately.	Appear- ance, approx- imately.	WAVE- NUMBER Spectrum Place per Brit. inch.	Differences.
Leading line of { 1st Compt. the band breaks } up thus:— { 2nd " 3rd " 4th "	1. 1.5 1.5 1.	— — — —	48,853 48,861 48,867 48,872	3 6 5 5	N.B. The expressions for "Intensity" are mere guesses, with no pretence to scientific- ally absolute value; and yet are often useful in giving some idea of the different relations (which may be most extreme) between two or more adjacent lines. A max. number 10 and a mi- nimum of 1 was at first inten- ded to be kept to; but the ex- quisite refinements of light in the spectroscope soon showed				
Very faint line	0.5	—	48,879	7					
Strong line	1.2	—	48,886	7					
Strong line	1.3	—	48,893	7					
Clear line	1.	—	48,900	10					
Very faint line	0.5	—	48,910	8					
Do.	0.5	—	48,918	5					
Do.	0.5	—	48,923	5					
Do.	0.5	—	48,928	5					

Close double line.. { 1st Compt. 2nd "	1. 1.		48,935 48,940	7 5
Very close double { 1st Compt. line { 2nd "	1.6 1.6		48,950 48,952	10 2
Single extra-bright line, con- sisting of two lines super- posed and forming the very visible "crossing-place" al- luded to in the paper, p. 103.)	3		48,969	17
Very close double { 1st Compt. line { 2nd "	1.5 1.5		48,988 48,993	19 5
Close double line.. { 1st Compt. 2nd "	1.3 1.3		49,010 49,018	17 8
Double line..... { 1st Compt. 2nd "	1.3 1.3		49,033 49,042	15 9
Double line..... { 1st Compt. 2nd "	1.3 1.3		49,062 49,069	20 7
Wide double line.. { 1st Compt. 2nd "	1.3 1.3		49,090 49,100	21 10
Wide double line.. { 1st Compt. 2nd "	1.3 1.3		49,120 49,133	20 13 20

me that there were many per-
ceptibly lower stages than what
I was quite contented at first
to regard as the *minimum visi-*
bile; and hence the subdivision
into tenths.

The *Wave-Numbers* for spec-
trum place are by no means
accurate to the last figure, es-
pecially for lines and haze-
bands of ultra-faintness; but
they are given just as resulting
from the micrometer observa-
tions, and will often indicate
for themselves, by the column
of differences (especially among
the linelets of this carbo-hydro-
gen blowpipe-flame's spectrum),
which observations have erred
unintentionally in excess and
which in defect—to minute
quantities, however, which do
not affect the grand matter now
under discussion.

The GREEN GIANT of CARBO-
HYDROGEN, or the leading line
of this flame-band

10.

49,174

6

Table (continued).

The Green Band of Carbon, beginning with its least-refracted or red-ward side : as seen in an end-on Carbonic-oxide vacuum-tube, illuminated by 1-inch sparks from a small induction-coil, and a little bichromate battery of 5 pots, each exposing 10 square inches of zinc.		The Green Band of Carbo-hydrogen, beginning with its least-refracted red-ward side : as seen in a coal-gas and air blowpipe-flame, viewed end-on.			
Object observed. (Bright lines or haze.)	Intensity, ap- prox- imately.	Appear- ance, ap- prox- imately.	WAVE- NUMBER Spectrum Place per Brit. inch.	Differences.	
Single line.....	1.3		49,153	7	
A weak abnormal band of exceed- ingly thin faint lines.....	0.5 0.5 0.5 0.5 0.5	 	49,160 49,168 49,171 49,175 49,180	8 3 4 5 4	
Thin hazy line	0.8		49,184	13	
Hazy line	0.8		49,197	15	
Hazy band line ... { 1st side } { 2nd " }	0.6	≡	49,212 49,219	7	
Hazy band line.....	0.6	≡	49,230 49,242	11 12 8	
Object observed. (Bright lines or haze.)	Intensity, ap- prox- imately.	Appear- ance, ap- prox- imately.	WAVE- NUMBER Spectrum Place per Brit. inch.	Differences.	
Hazy linelets so close together as to appear-ly coalesce and defy positive in- dependent mea- sure.	0.7 0.7 0.7 0.7 0.7 0.7	 	49,180 49,182 49,184 49,186 49,189 49,192	2 2 2 3 3 4	
Haze-linelets, still so close as to be inferred only	0.7		49,196	5	
Do. do.	0.7		49,201	4	
Do. do.	0.7		49,205	3	
Do. do.	0.7		49,208	9	
Do. do.	0.7		49,217	4	
Haze-linelets, still too close for sure measure	0.7		49,221	4	

Hazy band line.....	0.6	{	49,250	9	Do.	0.7	49,225	5
Very faint haze between ...	...	::	49,259	11	Do.	0.7	49,230	7
Hazy band line.....	0.5	{	42,270	12	Do.	0.7	49,237	6
Fainter haze between	...	::	49,282	15	Haze-linelets, still too close	0.7	49,243	7
Hazy band line	0.5	{	49,297	20	Do.	0.7	49,250	5
Fainter haze between	...	::	49,317	11	Do.	0.7	49,255	9
Hazy band line ... { 1st edge }	0.5	{	49,328	16	Haze-linelets, still very close	0.7	49,264	7
Fainter haze between	...	::	49,344	6	Do.	0.7	49,271	6
Hazy band line.....	0.4	{	49,350	14	Do.	0.7	49,277	5
Fainter haze between	...	::	49,364	10	Do.	0.7	49,282	8
Hazy band line.....	0.4	{	49,374	13	Haze-linelet	0.7	49,290	10
Fainter haze between	...	::	49,387	7	Do.	0.7	49,300	12
Hazy band line.....	0.4	{	49,394	11	Do.	0.7	49,312	10
Fainter haze between	...	::	49,405	12	Do.	0.7	49,322	9
Hazy band line.....	0.4	{	49,417	8	Haze-linelet, wider and more distinct	0.6	49,331	11
Fainter haze between	...	::	49,425	13	Do.	0.6	49,342	12
Hazy band	0.3	{	49,438	17	Do.	0.6	49,354	11
Fainter haze between	...	::	49,455	10	Do.	0.6	49,365	9
Broader hazy band	0.3	{	49,465	27	Do.	0.6	49,374	18
Fainter haze between	...	::	49,492	6	Haze-linelet, still wider	0.6	49,392	13
							49,405	15
							49,420	20
							49,440	15
							49,455	17
							49,472	15

Table (continued).

The Green Band of Carbon, beginning with its least refracted red-ward side : as seen in a coal-gas and air blowpipe-flame, viewed end-on.					The Green Band of Carbon, beginning with its least refracted or red-ward side : as seen in an end-on Carbonic-oxide vacuum-tube, illuminated by 1-inch sparks from a small induction-coil, and a little bichromate battery of 5 pots, each exposing 10 square inches of zinc.				
Object observed. (Bright lines or haze.)	Inten- sity, ap- proximately.	Appear- ance, ap- proximately.	WAVE- NUMBER Spectrum Place per Brit. inch.	Differences.	Object observed. (Bright lines or haze.)	Inten- sity, ap- proximately.	Appear- ance, ap- proximately.	WAVE- NUMBER Spectrum Place per Brit. inch.	Differences.
Haze-linelet	0.6	—	49,487	13	Linelet in haze	0.2		49,498	12
Do.	0.6	—	49,500		Do.	0.2		49,510	12
Second line in this flame's green band	5.	—	49,510	10	Do.	0.2		49,522	6
Haze-linelet	0.6	—	49,519	9	Do.	0.2		49,528	15
Do., suspected	0.6	—	49,529	10	Do.	0.2		49,543	24
Do.	0.6	—	49,540	11	Fainter haze between	0.2		49,567	
Do.	0.6	—	49,553	13	Faint haze-band	0.2		49,598	31
Do.	0.6	—	49,570	17	Fainter haze between	0.2			
Do.	0.6	—	49,583	13	Faint haze-band	0.2		49,621	23
Do.	0.5	—	49,596	13	Fainter haze between	0.2			
Do.	0.5	—	49,610	14	Faint haze-band	0.2		49,650	29
Do.	0.5	—	49,625	15	Fainter haze between	0.2			
Do.	0.5	—	49,643	18	Faint haze-band	0.2		49,682	32
Do.	0.5	—	49,668	25	Fainter haze between	0.2			

XIV. *Note on an Equation in Finite Differences.*

By J. J. SYLVESTER*.

I GAVE a great many years ago in this Magazine the integral of the equation in differences

$$u_x = \frac{u_{x-1}}{x} + u_{x-2},$$

which I obtained by observing that the equation could be solved by supposing each u of an odd order to be equal to the u of the order immediately superior, and also by supposing it to be equal to the u of the order immediately inferior. The upshot of the investigation expressed in the simplest language was to furnish two particular integrals of which one gives rise to the series

$$u_0=1 \quad u_1=1 \quad u_2=\frac{1}{2} \quad u_3=\frac{1}{2} \quad u_4=\frac{1 \cdot 3}{2 \cdot 4} \quad u_5=\frac{1 \cdot 3}{2 \cdot 4} \dots,$$

the other

$$u_0=1 \quad u_1=2 \quad u_2=2 \quad u_3=\frac{2 \cdot 4}{1 \cdot 3} \quad u_4=\frac{2 \cdot 4}{1 \cdot 3} \quad u_5=\frac{2 \cdot 4 \cdot 6}{1 \cdot 3 \cdot 5} \dots$$

See also Boole's *Finite Differences*, 2nd Edition (edited by Mr. Moulton), p. 235.

Now let ϕ , a function of any letter t , be the generating function of u_x . Then, since

$$xu_x - (x-2)u_{x-2} - u_{x-1} - 2u_{x-2} = 0,$$

we shall have

$$(1-t^2)\frac{d\phi}{dt} + (-1-2t)\phi = C;$$

and integrating we find

$$(1-t)^{\frac{3}{2}}(1+t)^{\frac{1}{2}}\phi = C \int dt \sqrt{\frac{1-t}{1+t}},$$

$$\text{or} \quad \phi = C' \frac{1+t}{(1-t^2)^{\frac{3}{2}}} + C \frac{\sin^{-1}t + \sqrt{1-t^2}}{(1-t)^{\frac{3}{2}}(1+t)^{\frac{1}{2}}},$$

$\frac{1+t}{(1-t^2)^{\frac{3}{2}}}$ we see at a glance gives the values of u_x corresponding to the first particular integral; and since the two first terms of the function multiplied by C are $1+2t$, it follows that this function is the generatrix of the second particular integral—in other words, that

$$\frac{\sin^{-1}t \sqrt{1-t^2}}{(1-t)^{\frac{3}{2}}(1+t)^{\frac{1}{2}}} = 1 + 2t + 2t^2 + \frac{2 \cdot 4}{1 \cdot 3} t^3 + \frac{2 \cdot 4}{1 \cdot 3} t^4 + \frac{2 \cdot 4 \cdot 6}{1 \cdot 3} t^5 + \dots$$

* Communicated by the Author.

Hence

$$\frac{t \sin^{-1} t + \sqrt{1-t^2}}{(1-t^2)^{\frac{3}{2}}} = \frac{1}{1+t} \left\{ t \right\} \frac{\sin^{-1} t + \sqrt{1-t^2}}{(1-t)^{\frac{3}{2}}(1+t)^{\frac{1}{2}}} \left\{ +1 \right\}$$

$$= 1 + \frac{2}{1} \cdot t^2 + \frac{2 \cdot 4}{1 \cdot 3} t^4 + \frac{2 \cdot 4 \cdot 6}{1 \cdot 3 \cdot 5} t^6 + \dots;$$

and integrating

$$\frac{\sin^{-1} t}{\sqrt{1-t^2}} = t + \frac{2}{1 \cdot 3} t^3 + \frac{2 \cdot 4}{1 \cdot 3 \cdot 5} t^5 + \frac{2 \cdot 4 \cdot 6}{1 \cdot 3 \cdot 5 \cdot 7} t^7 + \dots$$

Thus we have the remarkable identity

$$\left(1 + \frac{1}{2} \tau + \frac{1 \cdot 3}{2 \cdot 4} \tau^2 + \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} \tau^3 \dots \right)$$

$$\cdot \left(1 + \frac{1}{2} \frac{\tau}{3} + \frac{1 \cdot 3}{2 \cdot 4} \frac{\tau^2}{5} + \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} \frac{\tau^3}{7} \dots \right)$$

$$= 1 + \frac{2}{1} \frac{\tau}{3} + \frac{2 \cdot 4}{1 \cdot 3} \frac{\tau^2}{5} + \frac{2 \cdot 4 \cdot 6}{1 \cdot 3 \cdot 5} \frac{\tau^3}{7} \dots$$

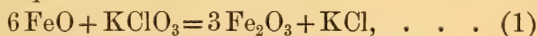
I do not recollect ever having met with these remarkable series before I discovered them by the preceding method; but on showing them to Dr. Story of this University, he ascertained that they had been stated not long ago by Mr. Glaisher in a paper in the 'Mathematical Messenger,' and made the foundation there of various summations for calculating π ; but where Mr. Glaisher found these series, which are not given in the ordinary books on the Calculus, or (if new) how he lighted upon them, he has not stated, and it is desirable that he should do so.

Johns Hopkins University.

26th May, 1879.

XV. *On the Laws of Chemical Change*—Part II. By JOHN J. HOOD, *Royal Exhibitioner in the Government School of Mines, London**.

WHEN ferrous sulphate in an acid solution is oxidized by potassic chlorate, the two salts being in the proportion required by the equation



I have shown† that the reaction which takes place may be very accurately represented by the algebraical equation $y(a+t) = b$, y being the residue of unoxidized iron after the action has continued for t minutes, and ab two constants.

* Communicated by the Author.

† Phil. Mag. Nov. 1878, p. 371.

In the experiments referred to, no account was taken of the hydric sulphate present in the solutions, it being merely stated that an indefinite quantity was employed. As this reaction seems likely to lead to some interesting facts regarding the effect of salts on rate of oxidation and the like, I have thought it advisable to study first the result of a variation in the amount and quality of the acid, in order to ascertain in what manner the constants in the above equation are connected with the conditions of experiment.

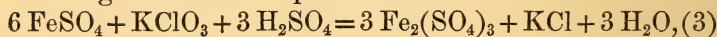
The equation $y(a+t)=b$ was established on the supposition that the amount of change in a unit of time was proportional to the product of the active bodies ferrous iron and potassic chlorate, and that these were in "equivalent" proportion according to (1).

Since hydric sulphate takes an active part in the oxidation, we may suppose from analogy that the amount of change in unit of time is proportional to the product of all three substances, viz., the iron $(A-\alpha)$, the potassic chlorate $(B-\beta)$, and the hydric sulphate $(C-\gamma)$, where A, B, C are the quantities of these bodies at the commencement of the action, $\alpha \beta \gamma$ the amounts that have suffered decomposition up to any time.

Consequently the equation representing this hypothesis will be

$$\frac{d\alpha}{dt} = k (A-\alpha) (B-\beta) (C-\gamma). \quad . \quad . \quad (2)$$

Taking the chemical equation to be



for equivalent quantities the following ratios will hold :—

$$\frac{A}{B} = \frac{\alpha}{\beta} = \frac{1}{\nu} \text{ (constant) ;}$$

$$\frac{A}{C} = \frac{\alpha}{\gamma} = \frac{1}{\nu'}$$

If, however, the acid be in large excess, say n times that required by (3), it may be considered to undergo comparatively little change. Treating it as constant and equal to $n\nu'A$, (2) becomes

$$\frac{d\alpha}{dt} = k\nu\nu'nA (A-\alpha)^2 ;$$

replacing $(A-\alpha)$ by y the residue of unoxidized iron, and integrating,

$$\frac{1}{k\nu\nu'nA} = y \left(\frac{c}{k\nu\nu'nA} + t \right).$$

Writing this equation in the more convenient form

$$b = y(a+t),$$

it is clear that, other conditions being the same, the constant b is inversely proportional to the amount of free acid, and a represents the number of units of time required to oxidize half the original iron. Experiments show a to have this value, as may be proved thus :—Let Y_0 be the value of y when $t=0$, then $b=Y_0a$. When y is reduced to one half, $Y_0a=\frac{Y_0}{2}(a+t)$; therefore $a=t$.

Experiments.—The iron solutions employed were prepared from ferrous sulphate which had been recrystallized twice, and contained about 2 per cent. ferrous iron and 10 per cent. hydric sulphate, together with a small quantity of ferric iron. When not in use they were kept in an atmosphere of coal-gas. The potassic chlorate was repeatedly recrystallized until free from chlorides and sulphates. The experiments were generally made in sets of four, two blanks containing always the same amount of acid, and two having their acid some multiple of that in the blanks. The mean values of the constants a , b , were then compared. In this way any slight fluctuations in the temperature of the water-bath were eliminated. The method of making the experiments was as follows :—After all the solutions had acquired as nearly as possible the temperature of the room, the required amount of hydric sulphate was run into a 250-cubic-centim. flask, the iron solution was next added, and the whole made up to the mark with distilled water that had previously been boiled to expel air. The mixture was then decanted into an ordinary flask of about 600 cubic centims. capacity and placed in the bath. When the four solutions had been made up in this manner, the potassic chlorate was run in with constant agitation and the time noted to two tenths of a minute; five minutes were added to this reading, and taken as $t=0$. The two observations from each solution required for the formula were taken, the first about 8 minutes after the addition of the potassic chlorate (or $t=3$), and the second when about 20 per cent. of the iron had been oxidized. The thermometer used was divided into tenths, and could easily be read to 0.5° ; all the burettes, pipettes, &c. were calibrated and expressed in terms of each other.

The first series of experiments (contained in Table I.) were performed with the following solutions—

Ferrous sulphate	{	ferrous iron 1.735 per cent.
		ferric " 0.030 "
		free H_2SO_4 9.86 "
Potassic chlorate		1.813 per cent. $KClO_3$.

Each experimental solution consisted of 28.7 cubic centims.

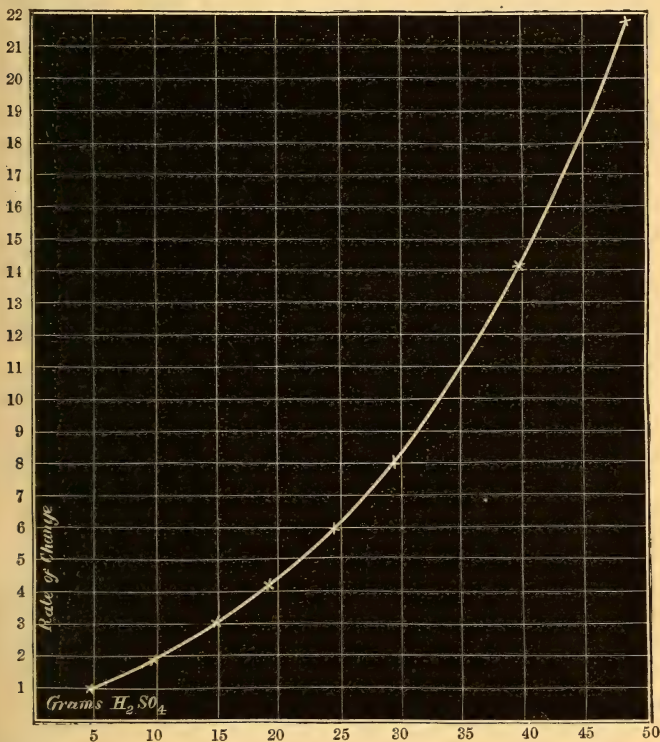
iron solution ($\cdot 4968$ grm. Fe), 10 cubic centims. potassic chlorate solution ($\cdot 1813$ grm. KClO_3), and the necessary amount of acid: the total volume in every case was 260 cubic centims. The permanganate used for determining the iron was of such a strength that 10 cubic centims. were equivalent to $\cdot 0199$ grm. Fe.

The numbers a and b in the Table were calculated by the equation $y(a+t)=b$, where y is the number of cubic centims. of the permanganate required for 10 cubic centims. of the experimental solutions, a' b' are the values for the blanks or when 5 grms. H_2SO_4 were used. The ratios are those of the means and the acid; that of $\frac{b}{a}$ gives the value of y when $t=0$.

Table I.

Temp. C.	a .	b .	Means.		Amount of H_2SO_4 .	Ratios.		
			a .	b .		$a' : a$.	$b' : b$.	Acid.
13°·8	243·5	2243·8	246·4	2269·2	grams. 5	1·910	1·950	2·00
	249·4	2294·7			"			
	129·8	1169·8	129	1163·6	10			
	128·3	1157·5			"			
12°·4	282·3	2598·5	282·1	2599·5	5	2·969	3·053	3·00
	281·9	2600·5			"			
	93·4	840·9	95	851·2	15			
	96·6	861·5			"			
12°·8	279·8	2554·0	275·8	2532·9	5	4·141	4·377	4·00
	271·8	2511·8			"			
	66·7	580·1	66·6	578·6	20			
	66·6	577·2			"			
12°·9	271·9	2502·2	271·4	2498·8	5	4·169	4·395	4·00
	270·9	2495·5			"			
	64·7	567	65·1	568·5	20			
	65·5	570			"			
13°·1	265·5	2448·9	265·5	2449·6	5	5·566	6·084	5·00
	265·6	2450·4			"			
	47·3	400	47·7	402·6	25			
	48·1	405·3			"			
11°·4	303·2	2809·3	305·3	2826·6	5	7·183	7·987	6·00
	307·5	2843·9			"			
	42·6	355·7	42·5	353·9	30			
	42·5	352·2			"			
13°	267·5	2485·2	269·6	2504·2	5	11·521	14·367	8·00
	271·7	2523·2			"			
	23·8	174·7	23·4	174·3	40			
	23	173·9			"			
13°·9	244·5	2255	244·1	2258·5	5	14·793	21·591	10·00
	243·7	2262			"			
	15·9	100·7	16·5	104·6	50			
	17·2	108·5			"			

By an inspection of the above Table it will be seen that from 5 to 20 grms. the rate of change is proportional to the amount of free acid, above 20 grms. the rate increases more rapidly than the acid. This latter fact seems strange ; for it might be supposed that when large quantities of acid were present the oxidation would suffer comparatively a retardation. That this increase of rate in proportion to the acid takes place gradually and, apparently, according to some definite law, is



shown in the curve in the figure, the ordinates representing the rate of change and the abscissæ the amount of acid.

Table II. contains the results of a series of experiments with the hydric sulphate, ranging from 3 grms. to 20 grms. Each experiment consisted of .4988 gm. Fe and .1820 gm. $KClO_3$, 10 cubic centims. of the permanganate being equal to .0192 gm. Fe ; total volume 260 cubic centims. The blanks are those containing 5 grms. H_2SO_4 , with which the other experiments are compared.

Table II.

Temp. C.	a.	b.	Means.		Amount of H_2SO_4 .	Ratios.		
			a.	b.		a' : a.	b' : b.	Acid.
9°·1	381	3731	334·5	3773·5	grams. 5	·637	·632	·600
	388	3816			"			
	601·7	5945			"			
	605·6	5992			"			
12°	293·6	2870·3	293·8	2882·6	"	·798	·796	·800
	294	2895			"			
	370·5	3651·8			"			
	365·8	3590			"			
10°·9	253·3	2480	251·6	2462·3	"	1·168	1·171	1·200
	250	2444·7			"			
	324·4	3184·4			"			
	324·9	3198·8			"			
11°	211·2	2039·6	211·6	2047·9	"	1·534	1·558	1·600
	212	2056·3			"			
	169·3	1632·8			"			
	166·4	1599·6			"			
10°·8	316·5	3115·5	319	3141·6	"	2·338	2·385	2·400
	321·5	3167·7			"			
	134·5	1300·3			"			
	138·5	1333·5			"			
10°·8	114·3	1090·7	114·3	1090·8	"	2·791	2·880	2·800
	114·4	1091			"			
	327	3246			"			
	331·7	3266			"			
10°·8	102·6	971·2	102·2	968	"	3·222	3·363	3·200
	101·8	964·8			"			
	77·2	721·9			"			
	78·2	726·9			"			

Somewhat similar results have been obtained from various chemical reactions. Harcourt and Esson investigated the influence of hydric sulphate on the rate of change when oxalic acid is decomposed by potassic permanganate*, and when hydric iodide is decomposed by hydric peroxide†; and they found that the rate of change depended on the acidity of the solutions. So also Boguski and Kajander‡, in their experiments on the evolution of carbonic dioxide from marble, found that the rate at which the gas was given off was proportional to the concentration of the acid.

Effect of Hydric Chloride.

A series of experiments were made to find how much hydric chloride was required to substitute a given weight of hydric

* Phil. Trans. 1866.

† Phil. Trans. 1867.

‡ Ber. deut. chem. Ges. 1877, p. 34.

sulphate to produce the same rate of oxidation as the latter acid alone. The values obtained would at first sight seem to represent the absolute ratio of the dynamical equivalences of these two acids; but on considering the different changes that take place in the two cases, such an inference cannot as yet be drawn from this single reaction. When hydric chloride is added to a solution of ferrous sulphate containing free hydric sulphate, the colour changes to a light straw-tint, owing probably to the formation of some ferrous chloride. When this solution is oxidized by potassic chlorate it becomes after a time of an orange colour, due to the ferric chloride formed; so that, when experimenting with two solutions, one containing hydric sulphate and the other hydric sulphate and chloride, in the former case we have ferrous sulphate being oxidized to ferric sulphate, and in the latter a mixture of ferrous sulphate and chloride (having probably different facilities of oxidation) converted into ferric sulphate and chloride, also an oxidation of the hydric chloride.

That ferrous sulphate in presence of hydric chloride is partly converted into chloride may account for the fact that in many cases when about 4 grms. HCl were employed the observed and calculated values for y or t were found to differ, the reaction seeming to undergo a gradual retardation. These discrepancies, however, might arise from the difficulty of making sufficiently accurate determinations of the iron by permanganate in presence of hydric chloride.

Various strengths of solutions were tried; and the numbers obtained for the ratio of HCl to H_2SO_4 were invariably the same within the limits of experimental error.

The most complete series of experiments, contained in Table III., were made with the following quantities of substances:—5440 grm. iron, 1985 grm. potassic chlorate, and various amounts of acid; total volume 260 cubic centims.; 10 cubic centims. of the permanganate equal to 0.0201 grm. of iron.

The numbers under “calculated ratio of dynamical equivalence of HCl to H_2SO_4 ” were obtained in the following way. It has been shown that b , or, in the first experiment with 6 grms. H_2SO_4 , 2357.5 is inversely proportional to the amount of acid; consequently to give the value 1664.8 it would require 8.498 grms. H_2SO_4 . But this number was obtained with 4 grms. H_2SO_4 and 2 grms. HCl . Hence by exchanging 4.498 grms. H_2SO_4 for 2 grms. HCl , the same rate of change would be produced. Expressed in molecular weights, 36.5 parts by weight of hydric chloride are equal to 82.1 parts of hydric sulphate in the dynamical sense.

Table III.

Temp. C.	a.	b.	Means.		Amount of		Ratio of H Cl to H ₂ SO ₄ .	Calculated ratio of dynamical equivalence of H Cl to H ₂ SO ₄ .
			a.	b.	H ₂ SO ₄ .	H Cl.		
10°·9	230·7	2349	231·3	2357·5	grms. 6	grms. X	2 : 2	36·5 : 82·1
	232	2366			"	X		
	163·6	1664·7	163·6	1664·8	4	2		
	163·7	1664·9			"	"		
12°·6	148	1487·7	148	1490·8	8	X	3 : 3	36·5 : 81·8
	147·9	1493·9			"	X		
	104·4	1023	103·3	1016·9	5	3		
	102·2	1010·8			"	"		
11°·5	218·8	2216·7	218·9	2225	6	X	1·108 : 2	36·5 : 80·9
	219	2233·7			"	X		
	201	2067·6	202·1	2067	4	1·108		
	203·2	2066·4			"	"		
15°	119·6	1195·5	120	1199·4	8	X	36·5 : 80	X
	120·4	1203·3			"	X		
	118·7	1194·5	119·4	1201·2	4	1·825		
	120·2	1207·8			"	"		
13°·9	168·6	1719·6	168·1	1714·4	6	X	36·5 : 80	X
	167·7	1709·3			"	X		
	167·1	1707·2	168	1708·7	4	·913		
	168·9	1710·3			"	"		
13°·2	93·9	927·4	93·8	927·6	12	X	36·5 : 80	X
	93·7	927·8			"	X		
	93·1	930	92·5	926·5	4	3·65		
	92	923			"	"		
* 14°·4	124·5	1116·3	124·1	1112·6	9·8	X	36·5 : 98	36·5 : 78·7
	123·7	1108·9			"	X		
	150·7	1374·4	151·6	1384·8	X	3·65		
	152·5	1395·2			X	"		

* These four experiments were made with different solutions from the others.

Taking into consideration the results contained in Tables I. and II. it is evident that the calculated dynamical ratio of hydric chloride to sulphate cannot be correct, since $\frac{1}{b}$ is not exactly proportional to the amount of acid. To find the true value a series of experiments were made with the hydric chloride to sulphate ranging from 36·5 : 75 to 36·5 : 85 ; and the numbers found to give the same rate of oxidation were 36·5 parts by weight of hydric chloride in place of 80 parts hydric sulphate (experiments nos. 4, 5, 6, Table III.). It is curious to note that this is the same as the ratio of the molecular weight of hydric chloride (36·5) to that of sulphur trioxide (80).

Several attempts have been made to obtain the ratio of dynamical equivalence of these two acids. Harcourt and Esson, with reference to their experiments on the decompo-

sition of HI by H_2O_2 , say*, “Comparing equivalent quantities, it had been observed that hydric chloride increases the rate of change nearly twice as much as hydric sulphate.”

Ostwald†, by a series of experiments on specific volumes arrives at the conclusion that $\frac{2\text{H Cl}}{\text{H}_2\text{SO}_4} = 1.93$.

Subsequently Mills and Hogarth‡, after a series of experiments on the effect of hydric chloride and sulphate on lactic acid, considered Ostwald’s result not far from the truth. Taking 80 as being the correct number for H_2SO_4 , the above experiments give for the ratio $\frac{2\text{H Cl}}{\text{H}_2\text{SO}_4}$ the value 1.63.

In comparing the rates of two different experiments by the equation $y(a+t)=b$ I have in every case taken the ratio $\frac{b}{\bar{y}}$ as representing the true value ; but, seeing that a is the time required to perform half the oxidation, $\frac{a}{a'}$ should be equal to $\frac{b}{b'}$. But this is not the case (Tables I. and II.). The reason of the discrepancies is, that the value of y in each case is not the same when $t=0$. If in two experiments we calculate the times required to reduce y , say, from 10 to 5 units, and compare them, we get the same ratio as $\frac{b}{b'}$.

It will be noticed that the constant a is independent of the strength of the permanganate used for determining the iron.

My best thanks are due to Dr. Frankland, in whose laboratory I had the advantage of performing the above experiments.

XVI. *Notes from the Physical Laboratory of University College, Bristol.* By Prof. SILVANUS P. THOMPSON, B.A., D.Sc. §

I. *On the Source of Sound in the Bell Telephone.*

THE question has been at various times and in sundry places discussed whether the sounds emitted by the Bell telephone, when used as a receiver of currents, are caused by molecular vibrations in the instrument, or are due to vibrations executed by the thin iron disk as a whole. The former

* Phil. Trans. 1867, p. 134.

† Journ. prakt. Chem. n. F. xvi. p. 419.

‡ Proc. Roy. Soc. vol. xxviii. p. 272.

§ Communicated by the Physical Society, having been read at the Meeting on April 26, 1879.

theory appears to have been started by Professor Bell himself* in order to account for the transmission of speech by instruments having very thick iron diaphragms, and by the instruments having no diaphragms at all. This view has been also upheld by the Comte Du Moncel in several communications to the learned societies of France. The other view appears to have been first distinctly put forward by Mr. W. H. Preece, in introducing the telephone to the British Association at Plymouth in 1877†; and it has for its most vigorous supporters M. A. Niaudet‡ and Colonel Navez, the latter of whom has replied more than once to points raised by M. Du Moncel. It is a view which appears also to be supported by the recent experiments of Professor Hughes.

The evidence now to be adduced, though not absolutely conclusive on the point at issue, opens out several fresh points of interest. It consists, in brief, of the results obtained by applying to the field of the telephone the experimental method of studying the so-called lines of force, originally due to Gilbert, and developed by Faraday. The details of the method followed by the present writer are identical with those described in his communication of June 23, 1878, "On Magnetic Figures," &c., and which consists in fixing permanently onto glass plates the figures obtained by iron-filings.

The figures obtained by means of iron-filings were resorted to with the view of ascertaining whether the changes in the magnetic field of the telephone were sufficiently marked to account for the alleged motions executed by the iron diaphragm, or whether they were such as to give any support to the molecular hypothesis.

The first step was to investigate the field of a bar-magnet when one pole was placed near a thin iron diaphragm.

It was known at the outset that a thin plate of magnetic matter might be magnetized in an enormous variety of ways. The magnetism might be distributed on the two faces, or in the manner known as *lamellar*; or, instead, any two points in the disk might be taken as conjugate poles; or any number of poles might be introduced; or, as in the magnets of M. Duter, the magnetization might be radially distributed, the central portion having one polarity, the other polarity existing all round the circumference. De Haldat showed that a variety of irregular magnetizations might be produced by touching

* A. Graham Bell, "Researches in Electric Telephony," Journ. Soc. Electr. Eng. 1878, p. 414, vol. vi.

† Rep. Brit. Assoc. 1877, Plymouth, p. 13, W. H. Preece, C.E., "On the Telephone."

‡ *Téléphones et Phonographes*, p. 92.

steel plates with the pole of a powerful magnet; and the present author also found analogous effects to be produced by passing currents through steel disks. It therefore became a matter of some interest to determine the character of the magnetization of the telephone-disk.

The figures exhibited two unsuspected features :—first, that when the diaphragm is larger than the end-face of the magnet, and even when it does not touch it, the distribution of the magnetism induced in the diaphragm is partially lamellar in character, partially radial. The central portion is magnetized almost entirely normally to its plane; the exterior portion is radially magnetized—a narrow annular region lying between these, in which the character of the magnetization is mixed. It was further observed that this neutral zone is of greater diameter when a larger magnet is employed, and that it enlarges also as the distance separating the magnet and the diaphragm is increased. It is more strongly marked as a region of separation between the central lamellar portion and the outer radially magnetized portion in the diaphragms of thin material than in those of thick. The position of this neutral annular zone is marked in fig. 1 (p. 132), which is a sectional diagram compiled from the figures produced by filings, by a point of flexure in one of the “lines of force” proceeding from the pole towards the diaphragm. The second feature noticed was that some of the outermost lines of force ran round to the front of the disk, entering it very near its circumference.

The next effect to be studied was that produced by the magnetic inductive action of a current traversing a coil of wire about the pole. For the convenience of obtaining the filing-figures upon glass plates but one turn of wire was employed, passing through holes drilled in the glass, and situated as is the coil in the Bell telephone over the pole of the magnet, the position arbitrarily found the most efficient in the construction of that instrument. In almost all modern elementary treatises on electromagnetics it is shown that the field of a plane closed circuit is equivalent to that of a lamellar magnetic shell of equal strength, or one which has an equal number of lines of force passing through the area it occupies. The result of passing the current around the pole of the magnet will therefore be, so far as the field *in the plane of the coil* is concerned, to increase or diminish respectively the number of lines of force due to the magnet by the number of lines of force due to the closed circuit, according as the direction of its field coincides with, or opposes that of the magnet. But the action is not so simple on the regions of the field outside the

plane of the circuit. The *direction* as well as the *number* of lines of force may be altered, and this in a manner so complex as almost to defy calculation, especially if the mutual induction between this magnetic combination and the adjacent iron disk be taken into account. The figures obtained with filings when the current traversed the circuit in opposite directions (see figs. 2 and 3) show that the lines of force proceeding



Fig. 1.

Fig. 2.

Fig. 3.

outwards from the pole were in fact thus altered both in number and in direction, and that, in addition to strengthening or weakening the field, the passage of the current had the effect of apparently thrusting the lines of filings forwards towards the iron disk or backwards from it. Moreover the region separating the two separate distributions of magnetism on the diaphragm was shifted on the passage of the current—

being contracted in diameter when the current reinforced the magnetism of the pole, becoming enlarged when it passed in the opposite sense.

Knowing from the experiments of Joule and De la Rive that a portion of iron, when magnetized in a particular direction, grows longer in that direction and shorter in its transverse dimension, let us deduce what the effect will be on the diaphragm of a telephone of these two species of magnetization. If the magnetization were radial, the tendency would undoubtedly be, supposing the disk clamped circumferentially, to thrust the middle point of the disk backwards towards the magnet, and to give it a conical shape. If the magnetization, on the other hand, were lamellar, the tendency would be to make the diaphragm thicker, and to contract it over the area thus magnetized. In the actual case where the magnetization partakes of both characters, the two distributions being separated by a neutral zone, the tendency to each form would exist over the regions respectively affected. But the extent of these regions varies with the varying induction of the currents in the coil. Hence, while the total attraction varies, giving rise to oscillations of the diaphragm as a whole, the neutral annular line will also be continually shifting its position and predisposing the diaphragm to take up new nodal forms of vibration, thereby rendering the timbre corresponding to the complicated undulations of the currents arriving from the transmitter.

The result obtained may be regarded from another point of view. If a slight displacement of the iron disk, though unable to affect to any appreciable extent the strength of the magnetic field as a whole, alters its strength at any one point or in any narrow region, or if, even without altering the average number of lines of force in any part of the field, such a displacement shifts the position of some of the lines of force across a narrow region of the field, it may still exercise a considerable inductive action on a closed coil of wire lying in the region where the amount of shifting is greatest. For, since the induced electromotive force in a closed circuit is not proportional to the strength on an invariable magnetic field in which it lies, nor yet to all changes in its strength, but only to such changes as cause a greater or less number of lines of force to pass through the area within the closed circuit, it is evident that the inductive action will be strongest in coils of wire which lie in the region where there is most change in the direction of the lines of force. We have here the *rationale* of the empirical practice of the constructors of the telephone alluded to above—namely, that of using only a small coil of

wire, and winding it upon a narrow bobbin placed upon the extremity of the magnet.

Conversely, the passage of a very feeble current through a coil so placed will produce a greater change in the effective intensity of the magnetic field between the core and the diaphragm than would be produced by the same current traversing a similar coil in any other region of the field ; for here it has its greatest power to shift the position of the neutral zone, and to alter the distribution of magnetism in the diaphragm.

It would therefore appear unnecessary to form an hypothesis of molecular vibrations in the disk to account for the emission of sounds by the instrument. Such vibrations do in fact exist ; but their existence does not necessarily prove that they play any important part in the production of the sound. And it must be remembered that, so far as the disk is concerned, they take place within the narrow range of the extreme positions possible to the neutral annular zone.

K Two further experiments seem to confirm the conclusion derived from the foregoing observations. If a compound diaphragm be used, consisting of concentric annuli of thin iron fixed to a stretched membrane of paper, or if a small iron disk thus fixed be employed, as in Bell's earliest experiment and in some of the experimental telephones of M. Niaudet, a curious timbre is thereby imported into the voices of speakers, though their enunciation is very distinct. A similar result is found to follow the employment of small thick diaphragms. In each of these cases the disposition favours the lamellar distribution of the magnetism.

If, however, a compound diaphragm be employed, consisting of a number of radial pieces similarly fastened to a stretched membrane, tones are well rendered, but enunciation is not distinct. This result is also obtained when the diaphragm of iron is too large in proportion to its thickness. In these cases the greater part of the magnetism is radially distributed.

Whenever a complete theory of the telephone is framed, these are points which must be taken into account. †

II. *On a new Variety of Magnetic Figures.*

De Haldat showed that it was possible to produce magnetic writing upon a steel plate by actually writing with the pointed pole of a powerful magnet, the writing being invisible until fine iron-filings were dusted over the plate. In the Physical Laboratory of University College, Bristol, a small circular saw has been found to afford a plate of suitable thickness and quality to produce good results. The latent characters remained for eight months after being inscribed.

While experimenting with these figures, it occurred to the author to try the effect of leading the current of a powerful battery into the plate and of writing on it with the other pole. This done, fine iron-filings were dusted over the plate; and on gently tapping it the writing became legible immediately. A small thin disk of steel which thus had a current passed through its centre exhibited afterwards a magnetism distributed in small concentric rings.

III. *On Magnetic Figures for Demonstration.*

For the production of magnetic figures filings of wrought iron are usually employed, though cast iron answers fairly. Finely powdered magnetic oxide is recommended by some writers, though it does not appear that its employment is attended with any great advantage. Professor A. M. Mayer took special pains* to produce filings of even quality from specially prepared Norwegian iron; but he says nothing about the size of filings he found best suited for the purpose. Faraday made the remark† that “large and also fine filings are equally useful in turn.”

The author, desiring to obtain figures on a larger scale than usual, for purposes of class demonstration, used a number of small steel needles with success. In the case of thin elongated bodies such as these, the magnetic moment is great as compared with the mass; hence it was to be expected that filaments of fine soft iron wire would also yield a good result. Accordingly he had a quantity of fine iron wire gauze of 32 meshes to the inch cut to fragments. The filaments thus produced were scattered in the usual way by means of a pepper-box with perforated lid. The figures given by these filaments with large magnets possess very well-marked characters, and are decidedly superior to those made with ordinary filings.

IV. *On the Magnetic Behaviour of fixed Iron Filings.*

Having occasion to draw the attention of his students to the property of the lines of magnetic force as being at every point tangential to the position of a small freely-suspended magnet placed above them, the author, placing thus a small magnet over the filings fixed some time previously to glass, and from which the magnet producing them had been removed, noticed that they still retained their magnetic property. It then occurred to him to see whether they still possessed directive force as a whole, and found that they appeared capable of attracting and repelling a lightly suspended needle. A figure

* *Vide American Journal of Science*, 1872.

† ‘*Experimental Researches*,’ vol. iii. p. 398.

produced by a single bar-magnet and fixed permanently to a slip of card by gum was suspended lightly upon a needle-point by means of a glass cap. It set itself in the magnetic meridian, and was found capable of being deflected on the approach of a steel magnet. The fixed magnetic curves are therefore themselves magnets.

V. *Magnetic Figures of three dimensions.*

The writer has several times essayed to produce magnetic figures of three dimensions. The difficulty in producing them arises from the weight of the iron filings when unsupported, as they must be when the whole figure does not lie in one horizontal plane. With even the most powerful electromagnets the forms of the curves cannot be actually constructed in iron filings for more than a few millimetres length.

Attempts to float iron filings are also difficult, as there is no transparent liquid nearly approaching the density of iron. The writer has tried heavy paraffins and strong solutions of mercuric iodide in potassic iodide. Better effects were obtained, however, when iron filings were employed which had previously been coated with shellac varnish, and which therefore possessed greater buoyancy. The experiment is curious; but the difficulty of seeing across the forests of lines of filings reduces the observation to one of curiosity only.

Another process attempted consisted in plunging a small magnet into a soft paste of plaster of Paris and iron filings. The plaster shortly hardened; and then sections were cut in various directions. The figures observed, however, were poor; and no observations were made of any additional interest as the result of the attempt.

XVII. *On the Dissociation of Aniline Colours.* By J. H. BICKET, Assistant in the "Young" Laboratory of Technical Chemistry, Glasgow*.

IN the course of their researches on dyeing, an account of which has recently been communicated to the Chemical Society†, it was found by Professor Mills and Mr. Thomson that a dilute aqueous solution of rosaniline acetate or hydrochloride is entirely decolorized by boiling, nevertheless imparting the rosaniline salt in its normal red state to a piece of silk immersed in the heated liquid. On Professor Mills's suggestion, I have made some further experiments in connexion with this subject.

* Communicated by Dr. Mills, F.R.S.

† 'Journal' (1879), i. p. 26.

I. *Rosaniline*.

300 cubic centims. of a solution of rosaniline acetate or hydrochloride containing .0003 grm. in a litre of water are completely decolorized by half an hour's heating to ebullition; 15 cubic centims. of this solution lose their colour in a few minutes. Either of these bleached solutions will then readily impart unaltered colouring-matter to a piece of immersed silk.

In order to avoid any possible bleaching effect due to any alkali that might be extracted from glass vessels by a boiling aqueous fluid, an experiment was made in a large platinum dish, with precisely the same results as before. We are therefore clearly dealing with a real case of dissociation, the rosaniline and its hydric acetate (or chloride) remaining in presence of one another without combination. In this respect these salts well maintain their known general analogy with sal-ammoniac.

In order to ascertain whether the dissociation would be reversed by prolonged preservation at the ordinary temperature, some of the bleached liquid was cooled, set aside, and observed from day to day. At the end of thirty days a considerable proportion of the colour somewhat suddenly returned.

The success of this experiment naturally induced me to try the effect of cooling the liquid, as a means of accelerating the return of the colour. Some of it was placed in a tube and cooled down to -17° C., when it of course solidified; the solid mass, when thawed, had a decided red colour.

The colour of the boiling liquid is restored, as might have been expected, by addition of a trace of acid. The temperature of boiling water appears to be that at which, in this case, complete dissociation is best effected.

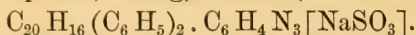
II. *Mauveine*.

A solution of mauveine acetate, of the same strength as in (I.), is bleached, by boiling, in much the same time as that of rosaniline. A solution of double that strength refuses to bleach, even when boiled for several hours. Silk immersed in the boiling decolorized liquid is immediately dyed. The colour can be restored, and the dissociation therefore reversed, either by freezing or by keeping at the ordinary temperature for several days; and the colour of the *boiling* liquid can be again brought out by addition of a trace of hydric salt.

III. *Nicholson's Blue*.

Unlike the preceding salts of amines, Nicholson's blue is a

sulphonic compound, having, as a rule, the formula



Our own sample, which was prepared many years ago, may possibly have contained a little of the disulphonic compound. The solution which I employed contained .1 grm. in a litre, being therefore three hundred times as strong as in I. and II. When this was boiled, even for several hours, it refused to bleach; but on allowing the hot liquid to cool gradually, the colour slowly faded away before the ordinary temperature was reached. Repeated attempts to decolorize the liquid by heating to temperatures short of 100° were all attended with failure.

When the decolorized liquid was kept for three or four hours, or frozen, or mixed with a little hydric acetate, the colour was restored.

The dissociation-temperature for Nicholson's blue is obviously a little below the boiling-point of water.

Phenomena of bleaching in connexion with Nicholson's blue were first noticed by my friend Mr. Louis Campbell.

XVIII. *On the Passage of the Galvanic Current through Iron.*
By FELIX AUERBACH, Ph.D., of Breslau.

[Continued from p. 18.]

§ 7. **I**N the following I will endeavour to elucidate the observed phenomena on the basis of the *theory of rotatable molecular magnets*. In so doing I make use of the conception of *work*, defining it for the present case as *the product obtained when the force which must be overcome for the rotation of a molecular magnet is multiplied by the angular quantity of that rotation*. The extra currents arising in iron have already been commonly recognized as the expression of such performance of work. When, from a fixed moment onwards, a current generated by a constant electromotive force performs work which till then it did not perform, then Ohm's law is valid only on the hypothesis that either the current-intensity i or the resistance w obtains another value. Assuming the former case, the equation is usually written

$$i = \frac{e - \frac{1}{a} \frac{\partial V}{\partial t}}{w};$$

that is, the quantity $\frac{1}{a} \frac{\partial V}{\partial t}$ (in which a denotes the value of the

* Helmholtz, *Die Erhaltung der Kraft*: Berlin, 1847.

work of the unit of heat, V the potential, for instance, of the unit current on the magnet in reference to which work is performed) is regarded as a new electromotive force which counteracts the first. But I do not see why it is not also admissible to write

$$i = \frac{e}{w + w_A},$$

where w_A denotes a new resistance added to w in consequence of the external work*. At all events it cannot be proved, as Colley† will have it, that the latter equation is false. Colley thinks himself authorized to conclude as follows:—If T and T' denote the times necessary, with and without the performance of work, to dissolve 1 gram of zinc in the galvanic series, then, if w varies,

$$\frac{e^2}{w} T = \frac{e^2}{w + w_A} T' + q T', \quad (1)$$

where q signifies the work done in the unit of time. Now, however,

$$\frac{e}{w} T = \frac{e}{w + w_A} T'; \quad (2)$$

consequently

$$e = e + \frac{q}{e} (w + w_A),$$

which is impossible.

Equation (1) is true; but equation (2) is as little applicable as in the other case (where e is considered variable) the corresponding equation

$$\frac{e}{w} T = \frac{e - \epsilon}{w} T', \quad (2A)$$

where ϵ signifies the variation of e . Much rather are both equations to be replaced by the common equation

$$T = T', \quad (3)$$

which expresses that with i the evolution of heat has also diminished; and this leads, in our case, to the very possible equation

$$\frac{e^2}{w} = \frac{e^2}{w + w_A} + q,$$

from which we get

$$w_A = \frac{w^2 q}{e^2 - w q} \quad (4A)$$

* But compare Wiedemann, *Galv.* ii. 2, p. 521.

† Pogg. *Ann.* clvii. p. 370 (1876).

and

$$q = \frac{e^2 w_A}{w(w + w_A)}. \quad . \quad . \quad . \quad . \quad (4B)$$

This equation can be readily verified if we pursue the extra current which arises at the closing of a known current, if the current deflects a magnetic needle of known moment, and if, besides, the horizontal component of the intensity of the earth's magnetism is known*.

In regard to *practice* it is at all events most convenient, whenever work is performed, whether momentary or lasting, to admit an *alteration of the resistance*.

Accordingly *the resistance of an iron wire in the first moment after the closing must be greater, in the first moment after the opening it must be less, than during the rest of the time that the current lasts*. For then the molecular magnets, in consequence of the directing force of the current, shift into a position more or less approximating to the circular arrangement when the current has to perform work in relation to the direction-force of the molecules. Here the molecules return more or less into their natural position; the direction-force therefore does work in regard to the current. With this the observations are in complete accordance.

As soon as that actual energy which the molecular magnets receive from the rotating force of the current is entirely converted into potential, the current has no more work to perform with respect to the direction of the molecular magnets. Hence we could not but conclude that the resistance would now take its true value, corresponding to the iron wire at rest internally (or in a determined thermal motion), if we had not to bear in mind that through the action of the rotating forces exerted by the current the internal state of the iron (as may also be imagined) has become different, and remains so till the current is interrupted. Accordingly the iron might possess two different resistances, of which one only, viz. that of the circularly mag-

* That even in the case represented by M. Colley (e variable) equation (2A) is not applicable may be inferred from its leading to a contradiction. For M. Colley arrives by correct conclusions at the equation (corresponding to eq. 4B)

$$q = \frac{\epsilon(e - \epsilon)}{w},$$

which cannot be true, since for ϵ indefinitely small it yields

$$q = \frac{e \, de}{w} \text{ instead of } q = \frac{2e \, de}{w};$$

while if in this case also, as above, we make use of eq. (3), it brings us to the last-mentioned, the true equation.

netized iron, would be at once accessible to observation, while the other, resistance of the unmagnetic, would have an important theoretic signification, inasmuch as it alone is comparable with the quantities which in other metals we bluntly call the resistance.

Assuming, then, that these two quantities indeed differ (and experience shows that this is the case), yet no way based on special conclusions can be imagined in which we could decide which is the greater. In such cases considerations having for their starting-point *the principle of the conservation of force* have recently been frequently applied with success. Taking the same course, I place first of all a general principle which results therefrom, and which hitherto, so far as is known to me, has been expressed only for special cases. It is:—

No force can of itself bring in conditions more favourable for its own action than those which it meets with.

This needs no explanation; even the expression “of itself” has become universally familiar since it was introduced by Clausius*. The proposition in which he first made use of it, “Heat can never of itself pass over from a colder into a hotter body,” is itself a special case of the above principle. Another is Lenz’s law which determines the direction of the induced current. Further, here belong the facts that in solids the cubic coefficient of compression diminishes as the pressure increases, that the specific heat and the galvanic resistance of solid bodies increase with the temperature, &c.

For the present case we may conclude from the above principle that the resistance of iron as observed by us when a current is conducted through it is greater than that ideal value. Thence, in the next place, it follows that circularly magnetized possesses a greater resistance than unmagnetic iron, at least if the amount of the circular magnetization does not exceed that which the current itself could produce. Evidently, however, we may drop this limitation; for if the circular magnetization be greater than the current itself can generate, yet there is always another intensity of current possessing this property. For this latter, therefore, the above proposition holds good. But the resistance of an iron wire *possessing a definite magnetic state*† is independent of the intensity of the current; consequently that principle is valid for any amount of circular magnetization.

It follows, further, that the resistance must be lessened by feeble longitudinal magnetizings. For, according to the above, we may assume that circularly magnetic iron conducts the

* *Die mechanische Wärtheorie*, i. p. 81.

† That this addition is necessary, will be shown in § 8.

current the worse the more intense the circular magnetization is ; but by the added longitudinal magnetization a portion of the circular is annulled. In fact my experiments show, in iron as well as in steel, a diminution of the resistance in consequence of feeble magnetizings.

The behaviour is different when the longitudinally magnetizing force is great ; a longitudinal magnetizing will then result from its cooperation with the circular magnetizing force of the principal current. And here the theory leaves a blank. The resistance of longitudinally magnetized iron can be less or greater than that of the unmagnetic. In the former case the resistance-function has nowhere a minimum or a maximum ; but rather *the resistance constantly rises from the state of saturated longitudinal magnetism to the state of saturated circular magnetism* : this case is realized, as the experiments show, in hard steel. *In the other case the resistance-function has a minimum value for the unmagnetic state* ; my experiments have in fact yielded this result in iron and soft steel : here, to one and the same ordinate of the resistance-curve two abscissæ correspond ; that is, two magnetic states (namely, longitudinally and circularly magnetic) correspond to the same resistance. The conjectural form of the resistance-curves is represented in Pl. I. fig. 2. This gives the explanation of experiments 7, 21, and 25, both so far as their result were positive, and also so far as they were negative. By making use of the laws laid down respecting magnetization by a circular current or by a spiral*, and respecting circular magnetization by the current flowing through the wire itself†, we can calculate what must be the ratio of the electromotive forces of the magnetizing and of the principal current (that is, the ratio $M : H$) at given values of n , w_0 , l , and d , in order that δ (for iron and soft steel) may vanish. But as the smallness of the values of δ hardly permits this calculation to be tested, I forbear to carry it out. I will only mention that, according to the experiments, that ratio appears to depend not only on n , w_0 , l , and d , but also on the nature of the wire. That the dependence on the thickness is considerable can be made evident by the following consideration. The longitudinally magnetic moment taken by the wire is, with equal magnetizing forces, nearly proportional to the square root of the thickness‡. The case is different with the circularly magnetic moment ; indeed, with equal magnetizing force the circularly magnetic moment appears not to depend essentially on the thickness : but the

* Conf. Wiedemann, *Galv.* ii. 1, pp. 180, 329.

† Streintz, *l. c.*

‡ Dub, *Electromagnetismus* (1861), p. 197.

magnetizing force varies considerably with the thickness ; for it is

$$P = c \int_0^{2\pi} \int_0^l \int_0^d \frac{r^2}{d^2} i dr dl d\phi.$$

Integrating, and using the equation

$$i = \frac{H}{w_k + \frac{4f}{\pi d^2} l},$$

in which f signifies the specific resistance of iron, we get for the magnetic moment :—

$$K = \frac{\text{const. } H l d}{w_k + \frac{4f}{\pi d^2} l} \cdot \phi(d) \cdot \psi(l),$$

where $\phi(d)$ is a complete function of d , slowly varying as d varies, and in the same direction, and $\psi(l)$ is a function of l which is of no consequence here. For larger values of l , at least if that of d is not too great, we can so arrange that w_k may be neglected ; we then find :—

$$K = \text{const. } H d^3 \phi(d) \cdot \psi(l).$$

But even when d is so great that conversely $\frac{4fl}{\pi d^2}$ can be neglected against w_k , still

$$K = \text{const. } H d \phi(d) \cdot l \psi(l),$$

while yet, as mentioned, the longitudinally magnetic moment is

$$K' = \text{const. } M n \sqrt{d} \chi(l).$$

As the expression of the work done by the current in the rotation of the molecular magnets, an extra current occurs at the closing of the principal current, as we have seen, or, as it was expressed, a passing augmentation of the resistance. If the wire has been previously magnetized longitudinally, and is still in that state at the closing of the principal current, the rotation by the latter is of course much less. From this we might at first be inclined to conclude that the work also is less, which would be contradicted by the observed fact that the extra current is in this case more intense. But we must bear in mind that the rotation is smaller in amount because the longitudinally magnetizing force holds back the molecules more strongly than the direction-force, which alone, in the first case, counteracted the force of the principal current, but the other factor of the product representing the work, the longitudinally

magnetizing force, is much greater than the direction-force. Now, as long as the rotations are infinitesimal, the force varies in inverse proportion to the angle of rotation; therefore the work of the current remains constant; but when the longitudinally magnetizing force is considerable, and therefore the rotations into the axial position cannot be regarded as infinitesimal, the work to be done by the current increases, although the circular turning produced by it is less. If, to demonstrate this, we denote by D the direction-force, by H the directing force of the principal current, we get (first, apart from a longitudinal magnetizing), for the work to be done for any one molecule in rotating it the angle ψ :—

$$A = \int_0^\psi D \sin \psi \, d\psi.$$

Now, if the direction of D for this molecule makes with the axis of the wire the angle ϕ , then ψ is determined by the equation

$$D \sin \psi = M \cos (\phi + \psi),$$

from which follows

$$\tan \psi = \frac{H \cos \phi}{D + H \sin \phi}.$$

Inserting this value in the equation

$$A = D(1 - \cos \psi),$$

we find

$$A = D \left(1 - \frac{D + H \sin \phi}{W} \right),$$

where W denotes the quantity $+ \sqrt{D^2 + 2HD \sin \phi + H^2}$.

If now we would describe rigorously the phenomena of the extra currents, we have to solve the following problems:—

(1) What is the mean value of A for all the molecules of the wire?

(2) What is the amount of the corresponding work for one molecule, on which, beside the forces D and H , the force M acts perpendicular to H ?

(3) What is the mean value of this work for all the molecules?

I have prosecuted this calculation under the following assumptions:—(a) In the unmagnetic state, all the values of

ϕ between 0 and $\frac{\pi}{2}$ are represented with equal frequency;

values between $\frac{\pi}{2}$ and π appeared to me, on account of the

unstable equilibrium which would then of necessity prevail in certain parts of the wire, very unlikely; moreover the value of the integral, so far as it comes into consideration, is independent of these. (b) The differences of direction of the molecules will quickly diminish with M increasing, consequently with diminishing deviation from the axis of the wire; that is, the integral which, divided by the difference of its limits, represents that mean value will be included within limits rapidly approaching one another, and at values of M which are great in comparison with D can be replaced by a differential, and consequently that quotient by a differential quotient; the final value of it then represents the extreme value of the work of the current for saturated longitudinal magnetism.

The work is, in the second case, greater than in the first, from two causes:—first, because the work increases with the force to be overcome; an understanding of this can be obtained, without a knowledge of those general expressions, in the following manner: we have

$$\frac{\partial A}{\partial D} = 1 - \frac{2D + H \sin \phi}{W} + \frac{D(D + H \sin \phi)^2}{W^3}$$

or

$$\frac{\partial A}{\partial D} = 1 - \frac{D + H \sin \phi}{W} + \frac{DH^2 \cos^2 \phi}{W^3}.$$

Now W can be written in the form

$$W = \sqrt{(D + H \sin \phi)^2 + H^2 \cos^2 \phi}.$$

Hence the sum to be subtracted from 1 in the expression for $\frac{\partial A}{\partial D}$ is greatest just when

$$D + H \sin \phi = H \cos \phi;$$

and in this case it is equal to

$$\frac{1}{2} + \frac{1}{2} = 1.$$

Thence it follows that $\frac{\partial A}{\partial D}$ is never < 0 . (We can also bring $\frac{\partial A}{\partial D}$ into the form

$$1 - \frac{(D + H \sin \phi)^3 + 2H^2 D \cos^2 \phi + H^3 \sin \phi \cos^2 \phi}{W^3},$$

from which we bring out the same conclusion by the development of W .)

But what has here been proved for $\frac{\partial A}{\partial D}$ holds also for $\frac{\partial A}{\partial M}$,

in case D is very small in comparison with M ; for the longitudinally magnetizing force is of the same nature as the direction-force. This is one reason why the work of the current for longitudinally magnetized is greater than for unmagnetic iron. But even when the force to be overcome is the same, the work is greater as soon as the angle between the direction of the force which does the work and the direction of the molecule at the commencement of the performance of work is greater. I will calculate at least the upper limit of this variation. Thus, let the molecule form with the axis the angle ϕ ; the first time let H act immediately and magnetize completely (that is, let the angle of rotation ψ be $=\frac{\pi}{2}-\phi$); let this work be A_1 . The next time let M act first and magnetize completely, and consequently rotate through ϕ ; then let H act and on its part magnetize completely, rotating through $\frac{\pi}{2}$; let this work be $\frac{A_2}{A_1}$. The ratio $\frac{A_2}{A_1}$ can, evidently, never become greater than in this case, in which M is of a higher order than D , H of a higher order than M . Now

$$A_1 = D(1 - \sin \phi), \quad A_2 = D;$$

therefore the mean values

$$[A_1] = \frac{2D}{\pi} \int_0^{\frac{\pi}{2}} (1 - \sin \phi) d\phi = \frac{\pi-2}{\pi} D,$$

$$[A_2] = \dots \dots \dots D;$$

that is,

$$\frac{[A_2]}{[A_1]} = \frac{\pi}{\pi-2} = 2.752 \dots$$

Therefore, if M increases, A increases more rapidly than M :—first, because, with continually greater accuracy, D can be neglected in comparison with M , and hence the above formula for $\frac{\partial A}{\partial D}$ becomes continually more strictly applicable; and, secondly, because the longitudinally magnetic state itself exerts a reaction, which, in rough approximation, can be taken into account by adding to A a factor constantly increasing in value with M from 1 to 2.752 Whoever, up to these data, examines the observations that have been made on the extra currents, will find them qualitatively (and, as far as this

is possible with the average insignificance of the deflections in general occurring, also quantitatively) verified.

I have characterized the extra currents in iron as the expression of the work of rotation, and described this work by an increase or a diminution of resistance. The value of the resistance corresponding to the closing current does not suddenly change into the value conditioned by the passage of a constant current or a permanent magnetization (or both together), but is mostly connected with it by a *phenomenon of afteraction*. Even of the nature of this, from the above an idea can easily be formed. If, beside the direction-force, only the principal current acts, it imparts to the molecules a rotation-velocity which, according to the value of H , is constantly positive (rotation) or periodically changes its sign (oscillation). Now, if even in a magnetic needle moving in a copper shell we observe a rapid conversion of the motion of the mass into thermal motion, the same conversion will result much more rapidly still in a molecule of an iron wire. A heating therefore takes place, which is only gradually equalized by conduction and radiation; and *the expression of this heating is the phenomenon of afteraction*. In fact, heat is generated not merely by longitudinal, but (as Villari* has shown), also by circular magnetizing. Into this I do not further enter; I will only mention that the series of experiments 16–18 of § 4 and the remarks (p. 16) in § 5 apply here. That in those experiments not only D and H , but also M acted, has, it is self-evident, no essential influence upon the result. I add a few numbers characteristic of the course of this kind of afteraction.

(1) $M=2B$, $H=1D$, $n=150$. These numbers were chosen so that δ was nearly $=0$. Wire f_4 . The principal current was closed after the magnetizing current. t denotes the time between two observations. On account of the considerable distance of the magnetizing current, some seconds mostly elapsed between the closing or opening of the magnetizing current and the first observation; hence the absolute alteration of resistance in consequence of the rotation-work cannot be inferred from the numbers. s denotes closing, δ opening of the magnetizing current. The ratios are graphically represented in Pl. I. fig. 3; the reinforced parts of the drawing correspond to afteractions.

It will be seen that at the opening the duration of the afteraction is less than at the closing.

* *Nuovo Cim.* (2) iv. Nov.–Dec. 1870.

<i>t.</i>		<i>w.</i>	Duration of the after-action.	
			At closing.	At opening.
		0.7906		
	<i>s</i>	0.7911	?	
	<i>ö</i>	0.7904	—	No afteraction.
	<i>s</i>	0.7912		
20"		0.7907	} 40"	
20		0.7905		
	<i>ö</i>	0.7903	—	} 20"
20		0.7905	—	
	<i>s</i>	0.7911		
15		0.7909	} 30"	
15		0.7908		
30		0.7908	—	
	<i>ö</i>	0.7905	—	35"
35		0.7906	—	35"
	<i>s</i>	0.7911		
30		0.7906	} 50"	
20		0.7905		
20		0.7905		
	<i>ö</i>	0.7903	—	} 30"
20		0.7905	—	
	<i>s</i>	0.7912		
40		0.7905	} 50"	
10		0.7904		
20		0.7904		
	<i>ö</i>	0.7904	—	No afteraction.
		Mean ...	45"	15"

(2) Without magnetizing. $H=2D$. A thick soft-steel wire, F_6 . The bridge in which was placed the galvanometer, the current being closed, and the current, the bridge being closed, were alternately closed and opened. In the first case there cannot in general be any extra current. (The weakening of the current in the wire by branching off into the galvanometer cannot, in my arrangement, have had any perceptible influence.) In the second case, on the contrary, the full current enters into the phenomenon. It was not powerful; nevertheless the afteraction was great. This was shown in the following way:—In the first case occurred a first excursion s_1 and a definitive deviation u_1 ; the two stood to one another in a constant ratio, conditioned only by the nature of the galvanometer-needle and the damping. In the second case an excursion s_2 occurred, greater than s_1 , and, just as the needle had finished its periodic oscillations, a deviation u_2 , likewise greater than u_1 , which sank only gradually to u_1 . In the Table, each row contains two experiments, viz. one of each sort.

$$w + w_k \text{ was } = 0.2389.$$

No.	s_1 .	u_1 .	s_2 .	u_2 .	Sinks to (u_1).
1.	4.3	2.3	5.2	4.0	2.6
2.	4.9	2.9	6.0	3.9	2.9
3.	5.2	3.0	6.1	3.9	?
4.	5.5	3.4	6.4	4.1	3.4
5.	5.6	3.5	6.6	4.1	?
Means...	5.1	3.0	6.1	4.0	3.0

Of the slow increase of all the numbers from experiment to experiment I shall speak presently.

Here belong also some facts already alluded to:—thus the phenomenon that, even in the cases in which the magnetizing diminishes the resistance, the first experiment gives the opposite result; for the evolution of heat occasioned by friction in the performance of work continues to operate. Further, the opening has often an influence in the opposite direction; that is, the resistance is lessened by magnetizing where otherwise it would be augmented, or is more strongly depressed than it would otherwise be. This phenomenon appears especially striking on the reversal of the current (compare § 4, experiment-series 10 and 11).

Some of the various phenomena just described may, I think, with more justice be designated as specific magnetic aftereffect*; but I here confine myself to a brief statement†. First, *the intensity of the extra currents increases at the frequent passage of the current, or at frequent magnetization*. This phenomenon has already been observed by Herwig; and he has explained it by an increasing mobility of the particles. The second of the above Tables shows it very clearly. At the same time it follows that the afteraction in consequence of the performance of work, which is the subject of that Table, does not simultaneously increase; for the numbers u_2 are nearly constant, and the difference $u_2 - u_1$ sinks from 1.7, through 1.0, 0.9, 0.7, to 0.6. In connexion with this is the fact *that the resistance of iron generally increases not inconsiderably on the current being repeatedly conducted through it*. This phenomenon must not be confounded with that observed by Von Quintus Icilius—namely, that the resistance of all metals is increased after a single long-continued passage of a current through them. The phenomenon is much more pro-

* I see, from a memoir by Fromme, just published (Wied. Ann. iv. p. 76), that he also uses the same expression for analogous phenomena.

† Conf. Herwig, Streintz, *l. c.*; and further, Herwig, Pogg. Ann. clvi. p. 430 (1875).

nounced in iron the first time it is used galvanically, and mostly vanishes after the tenth to the hundredth closing. Thus the result with an iron wire ($l=1670$, $d=0.20$, $H=2D$, $M=1D$) was:—

Initially	$w=7.3501$
After the 10th closing . .	7.3539
„ 100th „ . .	7.3552 (const.)
Longitudinally magnetized .	7.3547
Magnetized ten times . . .	7.3541
Demagnetized	7.3559

These numbers show the afteraction with transverse magnetization by the current as well as with longitudinal magnetization. That the decrease of resistance in consequence of the latter appears so trifling (it became much greater at equal values of H , M , n after the relations of the wire had become stationary), evidently depends on this—that the two afteractions, as indeed is mostly the case with these phenomena, do not go on independently of one another, and therefore, in consequence of the accelerated afteraction of the frequently repeated transverse magnetizing, a part of the decrease of resistance is concealed.

According to the preceding, it appears a probable supposition that *the permanent alterations of resistance with the magnetic state, produced by magnetizing, may also be taken as the expression of certain performances of work*. In fact, on a current passing through an iron wire, constant *rotations of the molecules* will be produced, which at an alteration of the arrangement, such as is produced by magnetizing, cannot remain unchanged. If these speculations are of a very hypothetical character, they nevertheless, I think, afford more fixed points than those of Beetz.

Attempts have been made by several to gain new fixed points by *artificial transverse magnetization* of the iron passed through by the current. I have not hitherto pursued this idea experimentally,—first, because we have not, as might be inferred from the hypotheses tacitly assumed by the authors in question, simply transverse opposed to longitudinal magnetization, but much rather here again, according to the arrangement, *many different kinds of magnetization* are conceivable (wherefore I have, for the sake of definiteness, designated the one here discussed as *circular magnetization*); and, next, because these experiments require still greater delicacy of measurement than experiments with longitudinal magnetization. The theory requires that artificial circular magnetizing (that is, additional to that produced by the principal current),

if it alter the resistance at all, shall augment or diminish it according as the direction of the artificial is the same as or opposite to that of the natural circular magnetization, and according to their intensities. I am not aware of any experiment on this point. On the other hand, Sir W. Thomson * conducted the current through a square iron plate in a direction inclined to the direction of the magnetization. The poles of the electromagnet were situated at two opposite sides of the square, the electrodes of the principal current in two opposite angles. Of the individual current-threads into which the plate under these circumstances divides, Thomson investigated the two meeting at the margin, each of which consists of two consecutive edges, A B C and A D C (fig. 4). The result of the experiment was, that the magnetization of the level-line D B was shifted into the position D ϵ , and consequently the resistance along A B was less than that along A D, and the resistance along D C was less than that along B C. Beetz and others have pointed out that even the mechanical pull connected with the magnetizing would by itself alone have necessitated this result. I think I must concur in this explanation; I will, however, show that my theory is not inconsistent with Thomson's observation, that, under some circumstances, it requires the latter, and that these circumstances were not present in the corresponding experiment made by Beetz † (which had a negative result).

Along the line A B, in Thomson's experiment, the directing force of the electromagnet will disturb the circular magnetization more or less, according to its intensity in comparison with that of the current-thread, and change it into a transverse magnetizing, in which all the molecular north poles point to the same side of the space. At the same time the resistance in certain lines of the current-thread, namely in those in which both the magnetizing forces have the same direction or make with one another an angle of 180° , will be but slightly changed; in the rest it will be generally diminished. Thus the total resistance will either be lessened or, if the electromagnet is powerful, somewhat but not much increased. But just in this latter case the resistance of the current-thread B C must very considerably increase; for here the electromagnet nullifies the circular magnetizing and produces a powerful longitudinal magnetization. From this, it is true, nothing can be inferred as to how the total resistance of A B C varies; but just as little can be inferred from Thomson's experiments. It merely follows that the whole or the greatest portion of any decrease of the resistance, but only

* *Loc. cit.* p. 741.

† *Loc. cit.* p. 206.

the smallest portion of any increase, must fall upon the part A B; and this we in fact learn from the above consideration. That I have therein taken no account of the action of neighbouring current-threads makes no difference; for the result of this action is, for A B exactly as for B C, that the molecular magnets are brought into a position a little nearer the perpendicular to the plane of the plate.

In Beetz's experiment the iron-wire spiral to be magnetized was inside the magnetizing copper spiral, so that the turns of the one were parallel to those of the other. Consequently the molecules were more or less approximately so placed that (supposing the windings horizontal and the current flowing in the copper in the direction of motion of the hands of a watch) all the north poles pointed downward. The principal current, on the other hand, called forth a circular magnetizing; therefore in the portions of wire belonging to the front half of the iron spiral, although the principal current flowed in the direction in which the hands of a watch move, the north poles in the anterior semicylinders into which each portion of the wire can be resolved were directed more or less upward, in the posterior (inner) downward. Conversely, in the portions of wire of the hinder half-spiral, the north poles in the posterior (outer) semicylinders were directed upward, and downward in the anterior ones. If, then, the magnetizing force of the principal current is not very little in comparison with the other, half of the current-threads present a stronger resistance than before the transverse magnetizing, the other half a weaker one; thus the total resistance remains nearly unaltered. The negative result of Beetz's experiment is therefore not surprising.

[To be continued.]

XIX. *On the Possibility of accounting for the Continuance of Recurring Changes in the Universe, consistently with the Tendency to Temperature-Equilibrium.* By S. TOLVER PRESTON*.

THE idea of the ultimate final cessation of all activity and life in the universe has been contemplated by many physicists with some dissatisfaction, and with the desire, if possible, to find some explanation or physical means by which so apparently purposeless an end is averted, and of avoiding the necessity for assuming in past time a violation of physical principles at present recognized to exist. The allied notion of an *unstable* universe whose parts tend to agglomerate together into one mass by successively falling together, would certainly, to

* Communicated by the Author.

say the least, appear to have something incongruous and unnatural about it, when regarded from a philosophical point of view; and however well grounded this aspect of the case might appear, still, from the vastness of the subject and the limited range of observation, it remains always conceivable that some physical link may have been left out, which affects the final conclusion.

As previous attempts to explain the phenomena of nature as the result of the action in the past of existing physical principles have been invariably welcomed, I venture to submit the following conclusion which has presented itself to me. I will commence at once by an illustrative case, noticing the difficulties as they arise. Let us imagine (for mere sake of illustration) a cubical envelope, which permits neither change of volume nor passage of heat, to enclose a space of diameter say 10^{10} times the distance between the Sun and Sirius. First, let the matter within this space be at the zero of temperature. Second, let all the matter within our envelope be at such a temperature that it is entirely dissociated into discrete molecules*. Between those two extremes there is room for any number of mean states in which matter might be more or less aggregated or discrete. Might not the universe actually be in one of these intermediate states? *i. e.* consisting of portions of matter in various stages of aggregation, moving among each other according to the principles of the kinetic theory, but not sufficiently rapidly to prevent gravity from producing a certain degree of aggregation. It should be scarcely necessary to observe that we have limited our space merely for the sake of fixing our ideas. All we require is gained if, instead of using the impermeable envelope, we surround our cube with infinite space filled with matter in a similar condition to that which the cube enclosed. There might perhaps be some who would find a difficulty at first sight in conceiving how two masses in translatory motion could collide without gravity making them coalesce and so the degree of aggregation continually becoming greater and greater. But it is to be noted that, if we imagine the masses to collide at such a limiting speed that the velocity with which

* The discrete molecules would of course be in motion, rebounding from each other in all directions, according to the principles of the kinetic theory of gases, and pervading the cubical unit of volume uniformly. Obviously we must take into account the neutralization of gravity within the cubical unit of volume, by realizing the space outside the cube filled with matter in a similar state to that which the cube encloses. The known tendency of the kinetic theory is automatically to produce a similar distribution of matter per unit of volume. Gravity acting from one unit of volume to another is thus neutralized.

the fragments or scattered parts rebound or glance off exceeds the greatest velocity of approach that gravity could generate in them when falling together from an indefinite distance, then the degree of aggregation after the collision would be *less* than it was before. Indeed, fixing the imagination upon a single cubical element of space containing detached solid masses of matter, and enclosed by a rigid envelope [other similar cubical elements existing outside—and the masses being figured for the instant at rest], it is then quite conceivable that such a velocity might be suddenly given to these detached masses of matter that the heat developed at their mutual collisions is sufficient to resolve the whole into discrete molecules (or a gas) pervading the cubical envelope uniformly, *i. e.* so that gravity is incompetent to produce any degree of aggregation (in the form of clusters or nuclei) at all*. Is not a less velocity than this conceivable which, when communicated to the masses, would still leave them some degree of aggregation, dependent on the mean velocity of translatory motion? which velocity (though constant from one unit of volume to another) would vary greatly from one mass to another in accordance with the principles of the kinetic theory—producing corresponding variations in the degree of aggregation from one mass to another without affecting the *mean* state (per unit of volume).

The case is comparable on smaller scale to the minute masses (each consisting of a number of molecules aggregated about a common centre) forming the compound molecules of a gas of high complexity. Here it is a known fact that such a velocity of translatory motion (dependent on temperature) might be given to these minute masses (compound molecules) composing the gas as to break them up into discrete molecules. A less velocity than this is conceivable at which a small degree of aggregation is possible, and a still less velocity where a still greater degree of aggregation can ensue. Indeed, if the constituents be numerous so as to admit of a great variety of groupings, a very considerable range in the degree of aggregation is possible by varying the rates of translatory motion (dependent on varying temperatures). It is also a recognized fact here that the degree of aggregation by any given rate of translatory motion (temperature) refers only to the *mean* state, and not to the state of each of the individual

* It is evident that since we are not limited as to velocity, a certain (adequate) velocity must exist, corresponding to an adequate degree of energy, that would suffice to produce this result. Of course the cubical envelopes are merely used for facility of illustration, and may be supposed abolished, substituting for them infinite space containing matter in a similar state.

masses ; for frequently one of the minute masses (molecular clusters) may acquire such a velocity in the accidents of collision as to break it up into discrete molecules at an encounter, these discrete molecules grouping together again in another part of the gas, the *mean* state of aggregation (per unit of volume) remaining unchanged. Of course it is obvious that there are differences of detail in considering the case of the masses of the universe. For example, in a compound gas, where the central agency producing aggregation is "chemical action," the fluctuation in size of a mass (molecular cluster) under the collisions is limited, whereas in the case of the masses of the universe, where the central agency producing aggregation is mainly "gravific action"*, the fluctuation in size

* The agency producing aggregation may even be found (when recognized) to be of the *same* kind in all cases ("gravific action," "chemical action," &c.). In former papers published in the Philosophical Magazine (Sept. and Nov. 1877, and Feb. 1878), I have called attention to the fact that, if the kinetic theory be applied to the motion of the particles of æther [in addition to that of the stellar masses suggested here], the gravitation of the molecules of gross matter may be accounted for under the ingenious fundamental idea contained in Le Sage's well-known theory, with the removal of all his postulates; or gravitation may be shown to be the necessary consequence of the mere immersion of the universe in finely subdivided matter moving automatically according to the principles of the kinetic theory. By the application of this theory to the stellar masses, molecules of gross matter, and particles of æther, as a vast whole, consisting of matter of different dimensions moving under its own dynamics, a grand dynamical generalization would evidently seem to suggest itself. It may be noted that, if we reject the now practically defunct conception of "*force*" in the sense of an "action at a distance" (without the intervention of matter), no other than a dynamical view of the universe is in principle conceivable.

Another consideration would seem to have an important bearing on this subject. The *parts* of the molecules of matter are known to possess a considerable capacity for motion. When, for example, the molecules of a gas are exposed to the pulsatory movement of the waves of heat (from some radiant source), the parts of the molecules are thrown into motion (vibration); and this development of motion in the parts is found to produce an accession of translatory motion in the molecules as wholes. This principle holds independently of scale; and thus it appears that the development of motion (by any means) in the constituent parts of a mass tends (under certain conditions) to produce translatory motion in the mass as a whole. If the æther, in which gross matter is immersed, be itself in a state of internal motion, this motion must inevitably communicate itself (to a certain extent) to the constituent parts of masses of matter immersed in the æther, and, accordingly, serve as a supplementary means to the development of translatory motion in the masses as wholes.

We may observe that, under the kinetic theory of gravity (based upon Le Sage's fundamental idea), both the range and intensity of gravity have a limit. The range (limited by the length of path of the particles) need not be necessarily much greater than that of which we have proof by observation, which is but a relatively small range compared with the stellar distances. The intensity of gravity therefore (for this reason) would not

would not be thus definitely limited. We are, however, dealing here with a fundamental matter of principle, not with subsidiary details*. It appears difficult to see where we are to limit the scale in the application of a principle, or how, if the kinetic theory be applicable to the case of a compound gas consisting of small masses (molecular clusters) of, say, as many as 50 to 60 molecules aggregated about a common centre, it should not apply when the *number* of molecules aggregated about a centre is increased (so as to form a visible mass). It might be said that the cases are different, inasmuch as the compound molecules of a gas rebound from each other as elastic bodies, whereas in the case of the masses of the universe they would generally be broken up and scattered at the encounters. This objection could only arise, however, from a superficial view of the case. For it is a known fact that the compound molecules of a gas often acquire in the accidents of collisions very great velocities, and they are thus broken up and their parts scattered at their encounters. There is, however, on the whole no work done (or loss of energy) in this breaking-up of the minute masses (compound molecules) of the gas; for the dissociated molecules unite again in another region of the gas; and so long as the *mean* state of aggregation (per unit of volume) in the gas remains unchanged, there is on the whole no work done. So in the case of the universe, if the *mean* state of aggregation remain the same, there would be on the whole no work performed by the occasional breaking-up of matter. But it may be said that at every such collision of two masses of the universe there would be a dissipation of energy in the æther attendant on the heat developed at the encounter, and this energy would be unavoidably lost. But if we regard the matter of the universe as (in the mean) uniformly diffused, as it would necessarily be under the kinetic theory, there would be no such actual loss of energy—merely a radiation backwards and forwards from one region to another. Thus in the smaller scale case of a gas, there is undoubtedly a dissipation of energy in the æther at every encounter of the small-scale masses (molecular clusters) in translatory motion; but this energy is (as is known) not lost, but only radiated to another

go on increasing *indefinitely* by the imaginary continual piling-up of matter, but would attain a final maximum. We allude to this last point as a possible detail of interest, without thereby implying that it essentially affects the main principle we have been developing in this paper.

* We merely apply in principle the same general mechanical considerations to molecules aggregated into clusters (lumps) under chemical action, as to molecules aggregated into lumps under gravific action (stellar masses).

region of the gas, the *mean* temperature (*per unit of volume*) of the gas remaining the same. One known consequence of the kinetic theory is that vast fluctuations of temperature may occur in the gas, from one of the portions of matter (consisting of one molecule or several) that moves as a whole in the motion of translation, to another portion, though the whole (reckoned by units of volume) may be in equilibrium of temperature. The same would apply in principle to the case of the universe, consisting of portions of matter in translatory motion among each other, if we do not limit the scale on which the principles are applicable; or it would follow that there might be vast differences of temperature from one portion of matter (stellar mass) to another, while the whole (*reckoned by units of volume*) might be in equilibrium of temperature. In order to have an idea, by inspection, of the mean state of temperature of the universe, we should require to sweep over a unit of volume, containing some hundreds of millions of stellar masses (dark and bright), in the same way as we do (on smaller scale) in examining the state of temperature of a gas; any appreciable volume of which contains some hundreds of millions of portions of matter in translatory motion (at different temperatures).

If it were possible to visualize the individual molecules in a compound gas at normal temperature, molecules would be observed in various parts glowing at a white or red heat (and some in a state of dissociation); and if the state of temperature of the whole gas were judged of from these relatively few *luminous* molecules, the assumption would be that the whole gas was at a white heat. So in the case of the universe, if the state of temperature of the whole were judged of from the perhaps relatively few *luminous* masses [which of course can alone be visible to us], an entirely false impression might be conveyed of the real state.

If therefore the above suggestion as to the possible application of an admitted dynamical principle on a large scale should be found valid, it would follow that the universe may already be at uniform temperature, in the sense that the limits within which there is fluctuation of temperature are indefinitely small compared with the collective universe, but that these limits are, relatively to a planetary system, very great, and amply sufficient to allow continual physical change, adapted to the maintenance of life.

So in an analogous way as regards the state of aggregation of the matter of the universe: since, by the application of the kinetic theory, this depends on the temperature, it would follow that the mean state of aggregation of the matter (per unit

of volume), like the mean temperature, is the same throughout—though indefinite fluctuations of dimensions would occur from one mass to another, in harmony with the fluctuations of velocity.

It would further follow from the principle that molecules of different densities (molecular weights) tend forcibly to become uniformly diffused, that by an adequate past duration of the universe the different kinds of matter must be uniformly diffused (per unit of volume) by the continued interchange of motion, though considerable fluctuations of mixture within ranges less than a unit of volume would be possible in harmony with the kinetic theory.

It may be observed that in principle, in order to account for the continuance of change in the universe, the existence of some process of *recurrence* is absolutely essential. The cooled down material of extinct suns must in some way be made available for the development of fresh suns or centres of heat. For if this were not the fact, there would be a continual accumulation of the material of extinct or useless suns in the universe, and processes of renewal and maintenance of the activity of the universe would come to a deadlock in the absence of matter to operate upon. It seems inconceivable how this end could be effected under recognized dynamical principles, excepting through an exchange of motion going on among the matter of the universe, involving collisions and an alternating renewal and loss of heat: or this seems on broad principle the only conceivable way in which there should be *recurrence*, under the condition that the *same matter should be used again*. There would appear to be a simple grandeur (not out of harmony with the recognized characteristics of nature) in this great result being brought about by the mere movement of the stellar masses according to the kinetic theory. Moreover the kinetic theory has been mathematically proved (when a large number of masses are concerned) to produce a system of order and symmetry (or *mean* similarity of the conditions in all parts of the system) which is rigidly and automatically maintained by a process of self-correction under dynamical principles—a self-acting adjustment of the motions continually taking place, whereby a system of harmony and order is maintained everywhere, a perfect state of mobile equilibrium existing in all parts.

Those who are inclined to view the physical causation of the past in the light of the physical causation of the present, or who look upon the principle of the conservation of energy as a truth as necessary in the past as in the present (or who are disposed to regard physical truths as independent of time),

are bound to believe that some process of recurrence must exist, whereby useful change and activity are continued in the universe, and the purposeless end of a changeless chaos prevented—and that we should seek for the explanation of this, not so much with the view to prove the fact thereby, but rather as a satisfaction or confirmation of a fact we already had logical grounds for believing to exist.

That recurring changes exist in the universe seems to have been the conviction of Sir W. Grove. He remarks relative to this subject ('Corr. of Physical Forces,' page 67):—"Enlarged observation may prove that phenomena seeming to tend in one direction will turn out to be recurrent, though never absolutely identical in their recurrence; that there is throughout the universe gradual change, but no finality; . . . that no star or planet could at any time be said to be created or destroyed, or to be in a state of absolute stability, but that some may be increasing, others dwindling away, and so throughout the universe, in the past as in the future."

Humboldt also says, as regards this point (Preface to 'Cosmos'):"I would therefore venture to hope that an attempt to delineate nature in all its vivid animation and exalted grandeur, and to trace the *stable* amid the vacillating ever-recurring alternation of physical metamorphoses, will not be wholly disregarded at a future age."

It has been pointed out by Dr. James Croll, in a paper published in the Quarterly Journal of Science for July 1877, that Helmholtz's gravitation theory of the origin of the sun's heat is not *alone* sufficient to account for a past duration of the sun's heat in harmony with geological evidence as to the age of the earth. It is of course evident that all the geological history of the earth must be comprised within the limit of the age of the sun. In this paper elaborate geological evidence is given tending to prove conclusively that as a *limit*, the age of the earth must be very much (at least three times) greater than the time the store of heat could have existed in the sun, if the origin of this heat were solely gravitation. For it is a known fact (based upon direct experimental evidence of the loss of solar heat by radiation) that the store of heat, if it had resulted from gravitation alone, would only have sufficed for about 20,000,000 years. Dr. Croll remarks regarding this point (page 317):—"We have not sufficient data to determine how many years have elapsed since life began on the globe, for we do not know the total amount of rock removed by denudation; but we have data perfectly sufficient to show that it began far more than twice 20 million years ago. Now in proving that the antiquity of our habitable globe must

be far greater than 20 or 30 million years, we prove that there must have been some other source in addition to gravity from which the sun derived his store of energy." The collision of matter in translatory motion is then suggested as the only conceivable other source of the store of heat-energy in the sun—this suggestion having already been made by the same author in a previous paper published in the *Philosophical Magazine* for May 1868, where it is remarked (p. 373):—"The Dynamical Theory of Heat affords an easy explanation of at least *how* such an amount of energy *may* have been communicated. Two bodies, each one half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would by their concussion generate in a *single moment* 50,000,000 years' heat" [*i. e.* an amount of heat which would cover the present rate of the sun's radiation for a period of 50,000,000 years].

It would seem to be scarcely realized what a field for rapidity of motion combined with all the stability or permanence of apparent rest the universe presents. It is a mere question of scale for bodies to possess any velocities (no consequence how great), and yet not alter their relative positions appreciably in a given epoch of time. Thus a stellar mass, for example, that moved transversely to the observer a distance equal to its own diameter [say a million miles, which is roughly the sun's diameter] in a second, would appear to the eye to be at rest; for the disk of the stellar mass has no apparent diameter even in the best telescope, and therefore the distance moved in a second would be invisible. Yet the velocity of the stellar mass in that case would be about five times that of light*. Dr. Croll has pointed out (*Phil. Mag.* July 1878) that

* Since light requires a quarter of an hour to traverse the diameter of the earth's orbit, and since that diameter (182,000,000 miles) would be a point when viewed at the distance of the nearest star, it follows that, if the nearest star were moving transversely to the observer with the velocity of light, the star might be watched for a quarter of an hour without appearing to deviate from its position. The actual angular distance traversed by the star, after the motion equal to that of light had been going on for a quarter of an hour, would be the thickness of a human hair held at 25 feet from the eye [this being the known representation of an angle of 2 seconds, which is that subtended by the earth's orbit at the distance of the nearest star]. This star might move with the velocity of light for $7\frac{1}{2}$ hours without traversing a greater angular distance than the thickness of a hair held 10 inches from the eye.

The tendency of modern science is unquestionably to look to a *dynamical* interpretation of phenomena in place of the old vaguely conceived statical ideas. The old tendency has been rather in the direction of ignoring the motions of the stellar masses, and of banishing from the conceptions (in the attempt to arrive at some notion of stability in the universe) all idea of a direct interference or mutual action of the moving parts of the

a star 1000 times more remote than α Centauri, though moving transversely to the observer at the rate of 100 miles per second, would take upwards of 30 years to change its position as much as $1''$ —in fact, that we should have to watch the star for a generation or two before we could be certain whether it was changing its position or not. It is evident that in regard to the motions of the stellar masses, some attempt must be made to realize their dimensions in order to have a just appreciation of the case. A motion that might seem harmonious and even slow in the case of a great ship (for example) would be utterly discordant and unfitting in the case of a child's toy boat. A stellar mass that moved through a fractional thousandth part of its own diameter in a second might be regarded as having a majestically slow motion; and yet if its diameter were a million miles [which may possibly be an average value], it would have traversed a thousand miles in that brief interval of time. These considerations may serve to show that if we apply the kinetic theory to the motions of the stellar masses, how completely different the case is from an ordinary gas as regards sequence of changes. The molecules of a gas, so far from merely traversing a space comparable to their own diameters in a second, are known as an actual fact to traverse a space equal to countless millions of times their own mean distance in a second [making roughly about ten thousand million collisions a second]; and yet, in spite of this rapid sequence of changes, the molecules, as regards absolute velocity, are almost at rest compared with the stellar masses. The scale is so incomparably different in the two cases*. Nevertheless, from the fact that the energy (heat) developed at the collisions depends on the absolute velocity, and of course not on the relative change of position of the masses, it becomes thereby possible by a sufficient scale to have an absolute velocity of any value however high, adequate to an enormous development of heat and explosive rebound † at the encounters, combined with so small a relative motion (or relative change of position) that the masses appear

universe upon each other. We, on the other hand, are led to take the diametrically opposite view, and to look to the dynamical action of the moving parts of the universe upon each other as the sole means of ensuring stability.

* While in the case of a gas the scale is too small for us to overlook directly the changes taking place in a unit of volume, in the case of the universe the scale is too big.

† The rebound would be far more than "explosive" in the ordinary sense of that term, since the expansive action of the heat developed by collisions even at moderate planetary velocities (calculably) far outrivals both in energy and suddenness the case of explosives. Indeed, if the colliding masses were formed of gunpowder, its ignition at the collision would add but little to the explosive energy of the recoil.

to the eye to be at rest. If it takes so long for the stellar masses to change their mean distances, what must it be in regard to the time taken to traverse their mean length of path (previous to the encounters); and thus in each individual case an almost limitless epoch of time is rendered possible for the conditions of life, while in the collective universe that stability and permanence are ensured which can alone rest upon recurring change.

In regard to a possible objection that, as far as observation has gone, the proper motion of the stars has not been found to exceed 30 to 50 miles per second, it may be replied that those stars whose proper motions it has been possible to estimate roughly, constitute an insignificant and almost vanishing minority compared with the rest of the visible stars, which are known to be situated at immeasurable distances. It has been pointed out by Dr. Croll that it would be scarcely reasonable to expect *luminous* stars to possess a high proper motion, since precisely they would have lost a greater part of their proper motion in the collisions which developed the heat which rendered them luminous, the proper motion having been lost by conversion into heat. Mr. Johnstone Stoney, in a paper published in the Proceedings of the Royal Society for 1868-69, has also dealt with the eventuality of collisions among the stars in a state of proper motion, and remarks (page 53):—"If what I here venture as a surmise with respect to the proximate cause of stellar heat and the origin of double stars is what really took place, we must conclude the sky to be peopled with countless hosts of dark bodies, so numerous that those which have met with such collisions as to render them now visibly incandescent must be in comparison few indeed" *.

The occasional appearance or blazing forth of "new stars," so notorious in astronomy, as if due to some sudden convulsion, would be in harmony with the view of collisions. That the stellar masses are in translatory motion, moving among each other in various directions and (in general) at such distances apart that gravity [if it exists at all at such distances] is incompetent to prevent the paths from being (sensibly) straight lines, is a well-established fact of observation. The application of the principles of the kinetic theory to the case would, therefore, seem to suggest itself rather in the light of a natural de-

* The fact of the present writer having arrived at the general conclusions enunciated in this article, before he had seen the papers of Dr. Croll and Mr. Johnstone Stoney, served rather to increase his confidence in the view he had adopted, and which he now ventures to suggest to the readers of the Philosophical Magazine.

duction than as a mere speculation. And even independently of all other considerations, it would seem more reasonable to look to a dynamical interpretation of the motions of the stellar masses, than to regard them as drifting indiscriminately among each other with the absence of all recognized purpose.

London, June 1879.

XX. On the Theory of Faults in Cables.

By OLIVER HEAVISIDE.

[Concluded from p. 74.]

16. **T**HERE is no difficulty in finding formulæ from the preceding results which will correspond to any particular example considered. Such formulæ, however, have, save to the mathematically curious, little value or interest unless they are interpreted numerically. Even then the labour involved is, save in special cases, out of proportion to the derived benefit. I shall confine myself to the simple cases of direct working without condensers, and with condensers, with a single fault in the centre of the line.

Suppose the signalling is made by means of a battery at P and a receiving instrument at Q, both of negligible resistance, and to earth direct. Then

$$0 = m_1 = m_2 = n_1 = n_2.$$

Let there be a single fault of resistance zkl at the centre of the line. Then

$$\tan b = 0,$$

$$\sin a + \frac{1}{za} \sin^2 \frac{a}{2} = 0,$$

by (10) and (11). The latter splits up into

$$\sin \frac{a}{2} = 0 \text{ and } \tan \frac{a}{2} = -2za. \quad . \quad . \quad . \quad (21)$$

Therefore, when i is even, $a_i = i\pi$; and when i is odd, a_i lies between $i\pi$ and $(i+1)\pi$. The denominator of (19) is

$$\begin{aligned} \phi(a, b) &= \int_0^{\frac{l}{2}} \sin^2 \frac{ax}{l} dx + \int_{\frac{l}{2}}^l \left\{ \sin \frac{ax}{l} + \frac{\sin \frac{a}{2}}{za} \sin a \left(\frac{x}{l} - \frac{1}{2} \right) \right\}^2 dx \\ &= \frac{l}{2} \left(1 - \frac{\sin a}{a} \right). \end{aligned}$$

Therefore, by (20),

$$A_i = \frac{2E}{a^i - \sin a_i}.$$

Hence the potential v at time t after the electromotive force E which produced the initial state is removed is, from $x=0$ to $x=\frac{l}{2}$,

$$v = 2E \Sigma \frac{\sin \frac{ax}{l}}{a - \sin a} \epsilon^{-\frac{a^2 t}{T}}; \quad . \quad . \quad . \quad (22)$$

and from $x=\frac{l}{2}$ to $x=l$,

$$v = 2E \Sigma \frac{\sin \frac{ax}{l}}{a - \sin a} \epsilon^{-\frac{a^2 t}{T}} + 2E \Sigma \frac{\sin \frac{a}{2}}{za} \frac{\sin a \left(\frac{x}{l} - \frac{1}{2} \right)}{a - \sin a} \epsilon^{-\frac{a^2 t}{T}},$$

which may be transformed into

$$v = 2E \Sigma \frac{-\cos i\pi}{a - \sin a} \sin \frac{ax'}{l} \epsilon^{-\frac{a^2 t}{T}} \quad . \quad . \quad . \quad (23)$$

(where $x' = l - x$) by making use of (21).

Let Γ be the current at Q. Then

$$\Gamma = \frac{2E}{kl} \Sigma \frac{-\cos i\pi}{1 - \frac{\sin a}{a}} \epsilon^{-\frac{a^2 t}{T}}.$$

If Γ_0 is the initial current,

$$\Gamma_0 = \frac{E}{kl \left(1 + \frac{1}{4z} \right)};$$

therefore

$$\frac{\Gamma}{\Gamma_0} = \frac{1 + 4z}{2z} \Sigma \frac{-\cos i\pi}{1 - \frac{\sin a}{a}} \epsilon^{-\frac{a^2 t}{T}}, \quad . \quad . \quad . \quad (24)$$

from which the arrival curve of the current may be calculated; for $1 - \frac{\Gamma}{\Gamma_0}$ is the proportion of the final current received at Q at time t after contact has been made with the battery at P.

17. The most easily calculated cases are $z = \infty$ and $z = 0$. When $z = \infty$, there is no fault, $a_i = i\pi$, (22) and (23) both become

$$v = \frac{2E}{\pi} \Sigma_1^{\infty} \frac{1}{i} \sin \frac{i\pi x}{l} \epsilon^{-\frac{i^2 \pi^2 t}{T}},$$

and (24) becomes

$$\frac{\Gamma}{\Gamma_0} = 2 \Sigma_1^{\infty} -\cos i\pi \epsilon^{-\frac{i^2 \pi^2 t}{T}}. \quad . \quad . \quad . \quad (25)$$

This equation (25) corresponds to curve 1, fig. 1 (p. 62), and is well known.

To find the limiting form of the arrival curve when $z=0$. By (23), when z is finite,

$$v = 2E\Sigma - \frac{\cos i\pi}{a - \sin a} \sin \frac{ax'}{l} \epsilon^{-\frac{a^2 t}{T}},$$

from $x'=0$ to $x'=\frac{l}{2}$. The initial potential v_0 between the same limits is

$$v_0 = \frac{Eax'}{l} \frac{4z}{1+4z}.$$

Therefore

$$\frac{v}{v_0} = \frac{l}{x'} \frac{1+4z}{2z} \Sigma \frac{-\cos i\pi}{a - \sin a} \sin \frac{ax'}{l} \epsilon^{-\frac{a^2 t}{T}}. \quad (26)$$

The $(2i-1)$ th and $2i$ th terms are

$$\frac{l}{x'} \frac{1+4z}{2z} \left(\frac{\sin \frac{a_{2i-1}x'}{l} \epsilon^{-\frac{a_{2i-1}^2 t}{T}}}{a_{2i-1} - \sin a_{2i-1}} - \frac{1}{2i\pi} \sin \frac{2i\pi x'}{l} \epsilon^{-\frac{(2i\pi)^2 t}{T}} \right),$$

where a_{2i-1} lies between $(2i-1)\pi$ and $2i\pi$, and ultimately becomes $2i\pi$ when z is indefinitely reduced, so that the last expression takes the form $\frac{0}{0}$. Evaluating in the usual manner, remembering that

$$z = -\frac{\tan \frac{a_{2i-1}}{2}}{2a_{2i-1}},$$

the $(2i-1)$ th and $2i$ th terms become

$$-2 \left(\cos \frac{2i\pi x'}{l} - \frac{4i\pi lt}{x'T} \sin \frac{2i\pi x'}{l} \right) \epsilon^{-\frac{(2i\pi)^2 t}{T}}.$$

Consequently (26) becomes, when $z=0$,

$$\frac{v}{v_0} = 2 \Sigma_1^{\infty} \left(\frac{4i\pi lt}{x'T} \sin \frac{2i\pi x'}{l} - \cos \frac{2i\pi x'}{l} \right) \epsilon^{-\frac{(2i\pi)^2 t}{T}}.$$

Now, when x' is indefinitely reduced, $\frac{v}{v_0}$ is the same as $\frac{\Gamma}{\Gamma_0}$; therefore, when $x'=0$,

$$\frac{\Gamma}{\Gamma_0} = \Sigma_1^{\infty} \left\{ \frac{(4i\pi)^2 t}{T} - 2 \right\} \epsilon^{-\frac{(2i\pi)^2 t}{T}} \dots \dots \dots (27)$$

From (27), curve 3, fig. 1, is calculated. The intermediate curve 2, fig. 1, for which $z=\frac{1}{4}$, is calculated from equation *Phil. Mag.* S. 5. Vol. 8. No. 47. Aug. 1879. N

(24). It is necessary in this instance, to first find the odd a 's from the second equation (21) and Tables.

18. Now for working with condensers at both ends. Let

$$m_1=0, \quad m_2=0, \quad n_1=\infty, \quad n_2=\infty,$$

and let r_1 and r_2 be both very small. At time t after the introduction of E at P , the potential of the line is

$$v=2Er_1\Sigma\frac{\cos\frac{ax}{l}}{1+\frac{\sin a}{a}}\epsilon^{-\frac{a^2t}{T}} \dots \dots \dots (28)$$

from $x=0$ to $x=\frac{l}{2}$, and

$$v=2Er_1\Sigma\frac{\cos i\pi\cos\frac{ax'}{l}}{1+\frac{\sin a}{a}}\epsilon^{-\frac{a^2t}{T}} \dots \dots (29)$$

from $x'=0$ to $x'=\frac{l}{2}$, where $x'=l-x$.

The a 's are the positive roots of

$$\sin a - \frac{1}{za}\cos^2\frac{a}{2}=0; \dots \dots \dots (30)$$

or

$$\cos\frac{a}{2}=0, \quad \tan\frac{a}{2}=\frac{1}{2za},$$

zkl being the resistance of the fault in the centre.

The current Γ arriving at $x=l$ is $\Gamma=r_2cl\frac{dv}{dt}$; that is,

$$\Gamma=\frac{2E}{kl}r_1r_2\Sigma\frac{-a^2\cos i\pi}{1+\frac{\sin a}{a}}\epsilon^{-\frac{a^2t}{T}} \dots \dots (31)$$

When there is no fault, $z=\infty$, $a_i=i\pi$, and equations (28) and (29) both become

$$v=Er_1+2Er_1\Sigma_1^{\infty}\cos\frac{i\pi x}{l}\epsilon^{-\frac{i^2\pi^2t}{T}} \dots \dots (32)$$

Here Er_1 is placed outside the Σ , because $a_0=0$, and the value of $\frac{1}{1+\frac{\sin a_i}{a_i}}$ is $\frac{1}{2}$ for a_0 and 1 for the rest. The current leav-

ing $x=0$ is $-r_1cl\frac{dv}{dt}$; or

$$\Gamma_{x=0}=\frac{2E}{k}r_1^2\pi^2\Sigma_1^{\infty}i^2\epsilon^{-\frac{i^2\pi^2t}{T}}; \dots \dots (33)$$

and the current arriving at $x=l$ is

$$\Gamma_{x=l} = \frac{2E}{kl} r_1 r_2 \pi^2 \sum_1^{\infty} -i^2 \cos i\pi \epsilon^{-\frac{i^2 \pi^2 t}{T}}. \quad (34)$$

19. To find the limiting forms of the solutions when $z=0$. In equation (29), when i is odd, $a_i = i\pi$; and when i is even, including 0, and z finite, a_i lies between $i\pi$ and $(i+1)\pi$, and ultimately becomes $(i+1)\pi$ when $z=0$. The $2i$ th and $(2i+1)$ th terms in (29) are

$$2Er_1 \left\{ \frac{\cos \frac{a_{2i} x'}{l}}{1 + \frac{\sin a_{2i}}{a_{2i}}} \epsilon^{-\frac{(a_{2i})^2 t}{T}} - \cos \frac{(2i+1)\pi x'}{l} \epsilon^{-\frac{(2i+1)^2 \pi^2 t}{T}} \right\}. \quad (35)$$

This vanishes when $z=0$, and (29) takes the form

$$v = 0 + 0 + 0 + \dots$$

each 0 representing a pair of terms. Now, when z is infinitely small

$$a_{2i} = (1-4z)(2i+1)\pi$$

by (30). Expanding (35) in powers of z , neglecting squares, &c., it becomes

$$2Er_1 z \cdot 4a \left\{ \frac{x'}{l} \sin \frac{ax'}{l} - \frac{1}{a} \cos \frac{ax'}{l} + \frac{2at}{T} \cos \frac{ax'}{l} \right\} \epsilon^{-\frac{a^2 t}{T}},$$

where a stands for $(2i+1)\pi$. The same result is reached by finding the limiting ratio of the expression (35) to z when $a_{2i} = (2i+1)\pi$, making

$$z = \frac{1}{2a} \cot \frac{a}{2},$$

and multiplying the result by z . Hence (29) finally becomes

$$v = 2Er_1 z \sum_0^{\infty} \left\{ \frac{4ax'}{l} \sin \frac{ax'}{l} + \left(\frac{8a^2 t}{T} - 4 \right) \cos \frac{ax'}{l} \right\} \epsilon^{-\frac{a^2 t}{T}}, \quad (36)$$

where $a_i = (2i+1)\pi$.

The potential v_2 of the receiving condenser is

$$v_2 = 16Er_1 z \sum_0^{\infty} \left\{ \frac{(2i+1)^2 \pi^2 t}{T} - \frac{1}{2} \right\} \epsilon^{-\frac{(2i+1)^2 \pi^2 t}{T}}; \quad (37)$$

and the current Γ entering the receiving condenser is

$$\Gamma = \frac{16E}{kl} r_1 r_2 z \sum_0^{\infty} \left\{ -\frac{(2i+1)^4 \pi^4 t}{T} + \frac{3}{2} (2i+1)^2 \pi^2 \right\} \epsilon^{-\frac{(2i+1)^2 \pi^2 t}{T}}. \quad (38)$$

Curve 3, fig. 2, is calculated from (37), and curve 3, fig. 3, from (38); curve 2, fig. 2, from (29), making $x'=0$; and

curve 2, fig. 3, from (31). In the last two $z=\frac{1}{4}$, and the even a 's are found by Tables.

20. The two important solutions

$$v = E \left(1 - \frac{x}{l} \right) - \frac{2E}{\pi} \sum \frac{1}{i} \sin \frac{i\pi x}{l} \epsilon^{-\frac{i^2\pi^2 t}{T}}, \quad . \quad . \quad (39)$$

and

$$v = Er_1 + 2Er_1 \sum \cos \frac{i\pi x}{l} \epsilon^{-\frac{i^2\pi^2 t}{T}}, \quad . \quad . \quad . \quad (40)$$

where, in (39), v is the potential at x at time t after the introduction of E at $x=0$, both ends being to earth, and in (40) v is the same when condensers of very small capacities $r_1 cl$ and $r_2 cl$ are interposed at the ends, there being no fault, may be both deduced from the corresponding formula when the condensers are of finite capacity. Suppose initially the condenser at $x=0$ to be charged to potential E , and the potential of the line and the condenser at $x=l$ to be zero, with no impressed electromotive force in the system. Then at time t the solution is

$$v = \Sigma A \sin \left(\frac{ax}{l} + b \right) \epsilon^{-\frac{a^2 t}{T}},$$

where

$$\tan b = -\frac{1}{r_1 a}, \quad \tan (a+b) = \frac{1}{r_2 a},$$

and therefore

$$\tan a = \frac{(r_1 + r_2)a}{r_1 r_2 a^2 - 1}. \quad . \quad . \quad . \quad (41)$$

Also

$$A = \frac{-\frac{Er_1 l \cos b}{r_1 a} + \int_0^l 0 \times \sin \left(\frac{ax}{l} + b \right) dx + 0 \times \frac{r_2 l \cos (a+b)}{r_2 a}}{r_1 l \frac{\cos^2 b}{r_1^2 a^2} + \int_0^l \sin^2 \left(\frac{ax}{l} + b \right) dx + r_2 l \frac{\cos^2 (a+b)}{r_2^2 a^2}}.$$

The result is

$$v = \frac{Er_1}{1+r_1+r_2} - 2E \Sigma \frac{r_1 a \sin \frac{ax}{l} - \cos \frac{ax}{l}}{1 + \frac{1}{r_1} (1+r_1^2 a^2) \left(1 + \frac{r_2}{1+r_2^2 a^2} \right)} \epsilon^{-\frac{a^2 t}{T}}, \quad (42)$$

where the constant term arises from the zero root of (41). Now, when $r_1=r_2=0$, the other $+$ roots of (41) are π , 2π , 3π , $\dots$; and (42) then becomes the same as (40). But when $r_1=r_2=\infty$, the roots are the same with the addition of a second zero root. In the general term of (42) make

$$r_1=r_2 = \frac{1 + \cos a}{a \sin a},$$

which follows from (41); and find the limit when $\alpha=0$. The result is

$$E\left(\frac{1}{2} - \frac{x}{l}\right).$$

This, added to $\frac{E}{2}$, what the constant term in (42) becomes when $r_1=r_2=\infty$, makes

$$E\left(1 - \frac{x}{l}\right),$$

which is the constant term in (39). The remainder of (39) is immediately deducible from (42) by making $r_1=r_2=\infty$.

21. The solution (40) for the potential in condenser working could be deduced from that for the *current* in working without condensers. For, in the latter case, the final result of the introduction of an electromotive force at $x=0$ is a current in the line of the same strength everywhere, and $v=0$ at $x=0$ and $x=l$; and in the former the final result is that the potential of the line is the same everywhere, and $\frac{dv}{dx}=0$ at $x=0$ and $x=l$. Both the current and the potential must satisfy the same partial differential equation. Hence the current in the latter case at x at time t must rise in the same manner as the potential in the former. Now

$$\gamma = \frac{E}{kl} \left\{ 1 + 2 \sum \cos \frac{i\pi x}{l} \epsilon^{-\frac{i^2 \pi^2 t}{T}} \right\}. \quad . \quad . \quad . \quad (43)$$

is the solution for the current in working without condensers, where $\frac{E}{kl}$ is the final uniform current. In the condenser-problem the final uniform potential is $\frac{Er_1 cl}{cl} = Er_1$, substituting which for $\frac{E}{kl}$ in (43), and changing γ into v , equation (40) results without a separate investigation. It is also very remarkable that (40) and (43) are capable of expression in an entirely different form, leading to the identity

$$\begin{aligned} & \epsilon^{-x^2} + \epsilon^{-(x-a)^2} + \epsilon^{-(x+a)^2} + \epsilon^{-(x-2a)^2} + \epsilon^{-(x+2a)^2} + \dots \\ &= \frac{2\sqrt{\pi}}{a} \left(\frac{1}{2} + \epsilon^{-\frac{\pi^2}{a^2}} \cos \frac{2\pi x}{a} + \epsilon^{-\frac{4\pi^2}{a^2}} \cos \frac{4\pi x}{a} + \epsilon^{-\frac{9\pi^2}{a^2}} \cos \frac{6\pi x}{a} + \dots \right) \end{aligned}$$

well known to mathematicians.

When $t=0$, the current as given by (43) is zero everywhere, except at $x=0$, where it is infinite; and in (40) the potential is zero everywhere when $t=0$, except at $x=0$, where it is infinite. These impossible infinite values arise from the

neglect of the battery-resistance in the one, and the condenser's capacity in the other instance. All mathematical investigations of physical questions are approximative; and being such, impossible results arise in extreme cases. If R is the battery-resistance, the current at $x=0$ when $t=0$ cannot be greater than $\frac{E}{R}$; but since there is always self-induction, the current, when $t=0$, is mathematically zero, rising in an extremely short time to $\frac{E}{R}$, and then falling to its final strength.

The actual rise of the current is more complex, on account of electromagnetic oscillations. Thus, from infinity we have got down to zero for the current at $x=0$ when $t=0$.

22. When we introduce the coefficient $s = \frac{L}{kl \cdot T}$, calculations become complicated by the presence of imaginary roots. That there must be imaginary terms in the solutions will be evident when it is considered that electromagnetic induction imparts inertia to the electric current, thus causing oscillations, and that

$$v = \Sigma A \sin\left(\frac{ax}{l} + b\right) e^{-\frac{a^2 t}{T}}$$

cannot contain oscillatory terms with real values of a . When there is a pair of terms in which A , a , and b are imaginary, their addition causes the elimination of the imaginary parts, and the result is real, as indeed it must be if the problem has physical reality. It is also evident that if in a physically real problem we have a single imaginary root, it must be of the form $a=0 \pm n\sqrt{-1}$, which makes a^2 real.

Taking a simple example, let the line be to earth direct at $x=0$, and to earth through a coil of resistance $mk l$ and electromagnetic capacity L at $x=l$. Also let there be initially a potential distribution

$$E\left(1 - \frac{x}{l(1+m)}\right)$$

in the line, and a current

$$\frac{E}{kl(1+m)}$$

through the whole circuit. This state would be produced finally by E at $x=0$. At $x=0$, $v=0$, and at $x=l$,

$$0 = v + ml \frac{dv}{dx} + sl^3 \frac{d^3 v}{dx^3}.$$

At time t ,

$$v = \Sigma A \sin \frac{ax}{l} e^{-\frac{a^2 t}{T}},$$

where the a 's are the + roots, including imaginary roots with + real parts, of

$$\frac{\tan a}{a} = -m + sa^2,$$

and

$$A = \frac{\frac{El}{a}}{\int_0^l \sin^2 \frac{ax}{l} dx - sla^2 \cos^2 a}$$

$$= \frac{2E}{a \left(1 - \frac{3 \sin 2a}{2a} - 2m \cos^2 a \right)}.$$

For simplicity, put $m=0$, then

$$v = \Sigma \frac{2E}{a \left(1 - \frac{3 \sin 2a}{2a} \right)} \sin \frac{ax}{l} e^{-\frac{a^2 t}{T}}, \quad \dots (44)$$

where

$$\frac{\tan a}{a} = sa^2. \quad \dots, \quad \dots (45)$$

When s is large, there is no trouble with imaginary roots. There is a root of (45) a little above zero, another a little under $\frac{\pi}{2}$; and the rest are nearly $\frac{3\pi}{2}, \frac{5\pi}{2}, \dots$. Hence, when s is large, (45) becomes

$$1 = sa^2$$

to determine the lowest root, or

$$a^2 = \frac{1}{s} = \frac{T \cdot kl}{L}.$$

Therefore (44) is nearly the same as

$$v = -\frac{Ex}{l} e^{-\frac{kl t}{L}} + \sum_1^{\infty} \frac{2E}{(i - \frac{1}{2})\pi} \sin \frac{(i - \frac{1}{2})\pi x}{l} e^{-\frac{(i - \frac{1}{2})^2 \pi^2 t}{T}},$$

and the current nearly the same as

$$\gamma = \frac{E}{kl} e^{-\frac{kl t}{L}} + \dots$$

This case corresponds to a short land-line, the self-induction of the receiving instrument causing greatly more retardation than the electrostatic capacity of the line. The current at

$x=l$ is always +. At $x=0$ it is first - for a very short time, and thereafter +. Except at first, the current is of the same strength throughout the circuit. Of the line's initial charge of potential $E\left(1 - \frac{x}{l}\right)$, a portion of potential E constant everywhere discharges quickly, nearly as if the line were insulated at $x=l$. The other part of potential $-\frac{Ex}{l}$ disappears exactly as the current decays, after the first moment. Or, more simply, the inertia of the current in the electromagnet causes the current at $x=l$ at any time to be stronger than it would have been without self-induction, in which case the current would be simply due to the line's charge. This charge, therefore, cannot supply enough electricity for the current; and the line becomes negatively charged, first at the end $x=l$, and afterwards all along. When this has happened the line-current is constant everywhere, and the - charge and + current die away uniformly.

As s decreases, the two roots of (45) lying between 0 and $\frac{\pi}{2}$ approach each other. When s reaches 1.47, they both become =1.1396, and simultaneously

$$1 = \frac{3 \sin 2a}{2a};$$

so that in the solution (44) the first term becomes $-\infty$, the second $+\infty$, their sum remaining finite. As s sinks below 1.47, the pair of roots become imaginary, and the first two terms of (44) may be put in a rather complicated mixed real form, indicating oscillations. When s reaches zero, the cable discharges in the ordinary way.

From (44) it follows that the potential at time t after introducing an electromotive force E at $x=0$ is

$$v = E\left(1 - \frac{x}{l}\right) - \sum \frac{2E}{a\left(1 - \frac{3 \sin 2a}{2a}\right)} \sin \frac{ax}{l} e^{-\frac{a^2 t}{T}}. \quad (46)$$

The electromagnet is here at $x=l$. Suppose now it is transferred to $x=0$, other things being the same; then instead of (46) we shall have

$$v = E\left(1 - \frac{x}{l}\right) + \sum \frac{2E \cos a}{a\left(1 - \frac{3 \sin 2a}{2a}\right)} \sin a\left(1 - \frac{x}{l}\right) e^{-\frac{a^2 t}{T}}. \quad (47)$$

Except when $s=0$, the permanent state of charge is arrived at in an entirely different manner in the two cases. v in (46)

is generally greater than v in (47) at any time. In the extreme, when s is large, the potential of the line according to (46) becomes nearly E everywhere, and afterwards settles down to $E\left(1 - \frac{x}{l}\right)$, thus,

$$v = E - \frac{Ex}{l} \left(1 - e^{-\frac{klx}{L}}\right) + \dots,$$

whereas according to (47) it rises, thus,

$$v = E \left(1 - \frac{x}{l}\right) \left(1 - e^{-\frac{klx}{L}}\right) + \dots$$

In spite, however, of this great difference in the phenomena of the charge, the current at $x=l$ rises in precisely the same manner in both instances, as will be seen on differentiating (46) and (47), and making $x=l$.

23. In the following example we have to deal with a single imaginary root. Suppose the line is initially charged to potential $\frac{Ex}{l}$, that the end $x=0$ is to earth, and that the current entering the cable at $x=l$ after $t=0$ is simply proportional to the potential there at any moment. That is, $v=0$ at $x=0$, and $v=ml \frac{dv}{dx}$ at $x=l$, where m is a + constant. At time t the solution is

$$v = \sum \frac{2E(m-1) \cos a}{a(1-m \cos^2 a)} \sin \frac{ax}{l} e^{-\frac{a^2 t}{T}}, \dots \quad (48)$$

where

$$\tan a = ma.$$

There is one particular case where the potential remains unchanged, viz. when $m=1$. All terms in the expression for v in (48) vanish except the first, for which $a=0$. The limiting value of

$$A \sin \frac{ax}{l} = \frac{2E(\sin a - a \cos a) \sin \frac{ax}{l}}{a(a - \frac{1}{2} \sin 2a)}$$

when $a=0$ is $\frac{Ex}{l}$; so that (48) is simply

$$v = \frac{Ex}{l}$$

when $m=1$. If m is greater than 1, v ultimately vanishes; but if m is less than 1, an imaginary root $a = n\sqrt{-1}$, where n is the + root of

$$\frac{\epsilon^n - \epsilon^{-n}}{\epsilon^n + \epsilon^{-n}} = mn,$$

comes into operation. The first term of (48) then increases with t without limit, the rest ultimately vanishing.

24. In general, the conditions imposed at the ends of a cable, when there are no impressed electromotive forces, are of the following form:—

$$\text{At } x=0, \quad 0=v+m_1l\frac{dv}{dx}+m_2l^2\frac{d^2v}{dx^2}+\dots; \quad . \quad . \quad (49)$$

$$\text{At } x=l, \quad 0=v+n_1l\frac{dv}{dx}+n_2l^2\frac{d^2v}{dx^2}+\dots \quad . \quad . \quad (50)$$

Here $m_1, \dots, n_1, \dots$, are constants, and v is the potential at any time.

Supposing there are no intermediate conditions, there is a single solution of the form

$$v=\Sigma A \sin \left(\frac{ax}{l} + b \right) e^{-\frac{a^2t}{T}}, \quad . \quad . \quad . \quad (51)$$

provided that the right-hand side of (51) can be made to satisfy (49) and (50), and to equal $f(x)$, an arbitrary function of x when $t=0$.

It follows from (49) and (50) that

$$\tan b = -\frac{m_1a - m_3a^3 + m_5a^5 - \dots}{1 - m_2a^2 + m_4a^4 - \dots}, \quad . \quad . \quad (52)$$

$$\tan (a+b) = -\frac{n_1a - n_3a^3 + n_5a^5 - \dots}{1 - n_2a^2 + n_4a^4 - \dots}; \quad . \quad . \quad (53)$$

and from these $\tan a$ can be expressed similarly, say

$$\tan a = -\frac{h_1a - h_3a^3 + h_5a^5 - \dots}{1 - h_2a^2 + h_4a^4 - \dots}; \quad . \quad . \quad (54)$$

and the a 's required are the $+$ roots, real and imaginary, of this equation.

Let

$$u = \frac{1}{l} \int_0^l \sin \left(\frac{a_1x}{l} + b_1 \right) \sin \left(\frac{a_2x}{l} + b_2 \right) dx,$$

where a_1, b_1, a_2, b_2 are any two pairs of values of a and b . Then, by integration,

$$u = \frac{a_1a_2}{a_1^2 - a_2^2} \cos (a_1 + b_1) \cos (a_2 + b_2) \left\{ \frac{\tan (a_1 + b_1)}{a_1} - \frac{\tan (a_2 + b_2)}{a_2} \right. \\ \left. - \frac{a_1a_2}{a_1^2 - a_2^2} \cos b_1 \cos b_2 \left\{ \frac{\tan b_1}{a_1} - \frac{\tan b_2}{a_2} \right\} \right\}. \quad . \quad (55)$$

Substituting in (55) the values of $\tan (a+b)$ and $\tan b$ from

(53) and (52), the bracketed quantities are always divisible by $a_1^2 - a_2^2$, and u is expressible as

$$u = r_1 \phi_1(a_1, b_1) \phi_1(a_2, b_2) + r_2 \phi_2(a_1, b_1) \phi_2(a_2, b_2) + \dots; \quad (56)$$

i. e. in the form of the sum of a number of products, each being a function of a_1 and b_1 multiplied by the same function of a_2 and b_2 and by a constant r .

Then assuming

$$E_1 = \Sigma A \phi_1(a, b), \quad E_2 = \Sigma A \phi_2(a, b) \dots,$$

it follows that

$$A = \frac{\frac{1}{l} \int_0^l f(x) \sin\left(\frac{ax}{l} + b\right) dx - r_1 E_1 \phi_1(a, b) - r_2 E_2 \phi_2(a, b) - \dots}{\frac{1}{l} \int_0^l \sin^2\left(\frac{ax}{l} + b\right) dx - r_1 \{\phi_1(a, b)\}^2 - r_2 \{\phi_2(a, b)\}^2 - \dots}. \quad (57)$$

When there are intermediate conditions, producing discontinuity in v or $\frac{dv}{dx}$ &c. at certain points x_1, x_2 , &c., each section must have its own series of the form (51). The a 's are the same for every section, being determined by the resultant of all the conditions. The A 's and b 's are different for each section. Thus

$$f(x) = \Sigma A \sin\left(\frac{ax}{l} + b\right) \text{ from } x=0 \text{ to } x=x_1,$$

$$= \Sigma A' \sin\left(\frac{ax}{l} + b'\right) \quad x_1 \quad x_2,$$

$$= \Sigma A'' \sin\left(\frac{ax}{l} + b''\right) \quad x_2 \quad x_3,$$

.

The intermediate conditions enable $A', b', A'', b'', \dots$ to be expressed in terms of A, a , and b . If

$$u' = \frac{1}{l} \int_0^{x_1} \sin\left(\frac{a_1 x}{l} + b_1\right) \sin\left(\frac{a_2 x}{l} + b_2\right) dx \\ + \int_{x_1}^{x_2} \frac{A'_2}{A_2} \sin\left(\frac{a_1 x}{l} + b'_1\right) \sin\left(\frac{a_2 x}{l} + b'_2\right) dx + \dots,$$

then u' may, as before in the case of u , be put in the form (56), and the value of A follows.

$$A = \frac{\frac{1}{l} \int_0^{x_1} f(x) \sin\left(\frac{ax}{l} + b\right) dx + \frac{1}{l} \int_{x_1}^{x_2} \frac{A'}{A} f(x) \sin\left(\frac{ax}{l} + b'\right) dx + \dots - \Sigma E r \phi(a, b)}{\frac{1}{l} \int_0^{x_1} \sin^2\left(\frac{ax}{l} + b\right) dx + \frac{1}{l} \int_{x_1}^{x_2} \sin^2\left(\frac{ax}{l} + b'\right) \left(\frac{A'}{A}\right)^2 dx + \dots - \Sigma r \{\phi(a, b)\}^2}. \quad (58)$$

The arbitrary quantities $E_1, E_2, \dots$ in (57) and (58), or rather, as many of them as turn out to be independent, are easily found to depend on the initial electromotive forces residing in those parts of the system in connexion with the cable, either at the ends or intermediate, which influence v at time t independently of its value $f(x)$ when $t=0$.

If, for example, we join two points x_1 and x_2 through a coil, its self-induction will introduce one E ; and if this coil have a closed circuit near it, a second independent E will be introduced.

25. Considering the line as of infinite length both ways, it will be found that if

$$v=f(x)=\Sigma A \sin \left(\frac{ax}{l} + b \right), \quad . \quad . \quad . \quad (59)$$

where the a 's are determined from

$$\tan a = - \frac{h_1 a - h_3 a^3 + h_5 a^5 - \dots}{1 - h_2 a^2 + h_4 a^4 - \dots}, \quad . \quad . \quad . \quad (60)$$

then will v satisfy the differential equation

$$\begin{aligned} & \left(1 + h_1 l \frac{d}{dx} + h_2 l^2 \frac{d^2}{dx^2} + h_3 l^3 \frac{d^3}{dx^3} + \dots \right) f(x+l) \\ &= \left(1 - h_1 l \frac{d}{dx} + h_2 l^2 \frac{d^2}{dx^2} - h_3 l^3 \frac{d^3}{dx^3} + \dots \right) f(x-l) \quad . \quad (61) \end{aligned}$$

everywhere, thus expressing the relation between the values of $f(x)$ at any two points separated by a distance $2l$. Or, which is the same thing,

$$0 = k_1 l \frac{dv}{dx} + k_3 l^3 \frac{d^3 v}{dx^3} + k_5 l^5 \frac{d^5 v}{dx^5} + \dots, \quad . \quad . \quad (62)$$

where

$$\begin{aligned} k_1 &= 1 + h_1, \\ k_3 &= \frac{1}{3} + \frac{h_1}{2} + h_2 + h_3, \\ k_5 &= \frac{1}{5} + \frac{h_1}{4} + \frac{h_2}{3} + \frac{h_3}{2} + h_4 + h_5, \\ &\quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}$$

In the particular case $h_1=0, h_2=0, \dots$, equation (61) reduces to

$$f(x+l)=f(x-l), \quad . \quad . \quad . \quad . \quad (63)$$

which simply expresses that $f(x)$ is periodic, repeating itself at intervals $2l$.

Starting from this equation, or an equivalent one, Mr. O'Kinealy (Phil. Mag. August 1874) proves Fourier's theo-

rem for periodic functions; that is, solving the linear equation (63), its solution is found to be

$$f(x) = \Sigma A \sin\left(\frac{i\pi x}{l} + b\right). \quad . \quad . \quad . \quad (64)$$

Hence it is concluded that an arbitrary function $f(x)$ may be expanded in such a series as the right-hand side of (64), though this proof of the possibility does not tell us how to do it. Mr. O'Kinealy, however, completes the solution in the usual way, leading to

$$\begin{aligned} f(x) = \frac{1}{2l} \int_0^{2l} f(x) dx + \frac{1}{l} \Sigma \cos \frac{i\pi x}{l} \int_0^{2l} f(x) \cos \frac{i\pi x}{l} dx \\ + \frac{1}{l} \Sigma \sin \frac{i\pi x}{l} \int_0^{2l} f(x) \sin \frac{i\pi x}{l} dx. \quad . \quad (65) \end{aligned}$$

Similarly, if we start from equation (61), which is linear, with constant coefficients, and includes the above case, we may easily prove that its solution is (59), with the condition that the a 's therein are the $+$ roots, real and imaginary, of (60), the A 's and b 's being undetermined. Or we may get the same result from (62), the a 's being now found from

$$0 = k_1 a - k_3 a^3 + k_5 a^5 - \dots \quad . \quad . \quad . \quad (66)$$

It will be observed that (60) or (66) have numerically equal $+$ and $-$ roots, each pair of which go to a single term of (59).

Here again the proof, if it may be now called a proof, gives us no information as to how to find the coefficients settling the amplitudes; and even the phases are undefined without further knowledge. But in working out practical problems requiring arbitrary functions to satisfy certain conditions when expanded in a harmonic series, the physical nature of a particular problem will usually suggest, step by step, the necessary procedure to render the solution complete, as in the last paragraph 24; and the completion of a solution is of far greater importance than any proof that the solution is possible.

With respect to the periodic series (65), it is only applicable to a cable when the ends are joined so as to make a closed circuit, changing $2l$ into l ; and there must be no external electrical connexions with the cable. If there are connexions at a point, or at several points, even without interrupting the continuity of the cable, although the potential of the cable will now repeat itself every time x is increased by l or $2l$ &c., yet the periodic form (65) will obviously not be suitable. The proper series are of course more general, and pass into the form (65) in limiting cases.

XXI. *Intelligence and Miscellaneous Articles.*

NOTE ON THE SPECTRUM OF BRORSEN'S COMET.

BY PROF. C. A. YOUNG, OF PRINCETON, N. J.

AFTER several unsuccessful attempts, I have at last, on April 1st and 2nd, obtained fairly satisfactory observations of the spectrum of this comet. It consists of three bands, like the spectra of all the other comets hitherto observed, the bands being well defined at the lower (least refrangible) edge and fading out towards the upper. The spectrum is so faint that observation is very difficult, and I was able to determine the position of only one of the bands—that in the green, which is much brighter than the other two.

The instrument employed was the 9½-inch refractor of our new observatory, armed with a one-prism spectroscope of sufficient dispersive power to separate the D lines clearly: the eyepiece has a micrometer which carries a bar thick enough to be seen on the background of even a very feeble spectrum. The observation was made by placing the bar so that the bright edge of the band should be just visible as a thin line, the rest of the band being occulted. The instrument has also a scale like that of the ordinary chemical spectroscope; and the position of the micrometer-bar is determined both by the reading of the micrometer-screw and by the reading of the scale, illuminated for a moment after the bar has been set.

On April 1st I got three scale-readings—respectively 99·9, 100·0, and 100·4, the value of one scale-division in this part of the spectrum being very nearly 25 units of Ångström's scale, or about double the distance between the extreme lines of the *b* group, the readings decreasing with the wave-length.

Just before dark, b_1 in the spectrum of daylight coincided with 100 on the scale; also, immediately after the third pointing and without disturbing the telescope, spectroscope, or micrometer, the flame of a Bunsen burner was presented to the slit; and the lower edge of the green band in the well-known spectrum of this flame was found to show itself at the edge of the occulting bar precisely where the comet-spectrum had been. We may therefore fairly conclude that the lower edge of the central band in the comet-spectrum had a wave-length of very nearly 517 millionths of a millimetre. The observation of April 3rd confirms this, though but a single reading could be obtained. The only special interest in this observation lies in the fact that in 1868 Mr. Huggins obtained a somewhat different result for this same comet.

In an elaborate paper published some years ago by Vogel in Poggendorff's *Annalen*, upon the spectra of comets, he comes to the conclusion that there are several different kinds of cometary spectra, the differences lying merely in the wave-length of the bands. But he seems to have reached this conclusion by assigning rather too high a degree of accuracy to the observations. With the exception of Brorsen's comet, it would seem that the discrepancies between the different results are entirely within the range of probable error, and that there is no valid reason for supposing

more than a single cometary spectrum, slightly modified in different comets by differences of pressure and temperature.

It would now appear from my observations that Brorsen's comet also must fall into line with the rest.

I am entirely at a loss how to explain Mr. Huggins's result. It can hardly be that the comet has really changed its spectrum in the meanwhile; and a careful reading of his account (*Proc. Roy. Soc.* vol. xvi. p. 388) gives no light as to how an error could have crept into his work; on the other hand, every precaution would seem to have been taken. However this may be, I am quite positive as to the accuracy of my present result—that the middle band of the spectrum of this comet now coincides sensibly (to a one-prism spectroscope) with the green band in the hydrocarbon spectrum.

The comet is moving very nearly in the path assigned by the ephemeris of Schulze. It is easily visible in the 3-inch finder of the equatorial, and in the telescope itself appears as a round nebula, between 30" and 40" in diameter, without definite nucleus, though much brighter in the centre. Before the new moon a faint tail was visible, about one half degree in length. It appeared like a thin streamer, much narrower than the head of the comet, perfectly straight, and directed from the sun.—*Silliman's American Journal*, May 1879.

ON STOKES'S LAW. BY M. LAMANSKY.

It is known that Stokes, in his important researches on fluorescence, laid it down as a principle that the refrangibility of the light emitted by fluorescence is less than that of the exciting rays.

Stokes's law has lately been called in question by Lommel, who in several memoirs published in the *Annalen der Physik* has sought to show that it is not a general law, and that there are cases in which the fluorescent light possesses a greater refrangibility than that of the incident light which excites the fluorescence. The results obtained by Lommel in his experiments have been confirmed by B. Brunner (of Prague) and Lubarsch (Berlin); but Hagenbach, the author of some very accurate studies on fluorescence, was not able to arrive at the same results as Lommel.

After repeating the experiments described in the memoirs of these physicists, it appeared to me necessary, in order to decide this controverted question and give an experimental proof of Stokes's law, to discover a method that would permit direct measurement of the refrangibility of fluorescent light and its comparison with that of the incident light exciting the fluorescence. For this purpose the light of the exciting rays operated on must be perfectly homogeneous. This can be attained by making use of the method first employed by Maxwell and Helmholtz, and afterwards by several other physicists. The following is the method I have used in these researches:—

The solar rays reflected by a heliostat were concentrated by an achromatic lens upon a slit, behind which were placed two flint-glass

prisms and an achromatic lens ; the latter was separated from the slit by twice its focal distance. This arrangement gave me a spectrum sufficiently pure for the principal lines to be seen in it. The spectrum was expanded on the side of a box in which was a movable slit, which could be shifted to the different parts of the spectrum, and the breadth of which could be modified at pleasure. Through this slit I let certain rays of the spectrum, which I previously caused to traverse a flint-glass prism, enter the box, which contains a vessel filled with fluorescent liquid. After this, by means of a reflecting prism I direct these perfectly homogeneous rays upon the fluorescent liquid. Between the surface of the liquid and the slit in the side an achromatic lens is placed, which produces the coloured image of the slit upon the surface of the fluid. With a second reflecting prism I direct the light which comes from the fluorescent liquid into the slit of the collimator of a Brunner's spectrometer. In the field of vision of the telescope of the spectrometer I get two coloured images—one produced by the fluorescent light, the other that which is directly reflected at the surface of the fluid. I then measure the minimum deviation of these two images.

I give the values I have obtained for fluoresceine :—

Incident light.		Fluorescent light.	
Width of the pencil.	Mean deviation.	Width of the pencil.	Mean deviation.
2 51	50 38	1 25	48 43
0 46	49 59	0 25	48 18
0 46	49 60	0 53	48 12
0 48	48 18	1 60	48 60
0 48	47 56	0 20	47 48

These experiments show that the fluorescent has a lower refrangibility than the incident light. I have repeated the same experiments with eosine, naphthaline-red, and chlorophyll, and obtained the same results.

In my researches I have taken the fluids at different degrees of concentration, and in layers of different thicknesses ; and the result has always been the same. Each fluid has in the spectrum certain rays which excite in it the strongest fluorescence ; with other rays the fluorescence will be feebler ; and it will disappear if rays still less refrangible be operated on. All the rays in the spectrum which are more refrangible than the fluorescent ones excite fluorescence in these fluids. It is upon naphthaline-red that I have obtained the greatest change of refrangibility of light, in which case the incident rays, whose index of refraction for flint-glass is 1.63917, were changed into rays having the refraction-index 1.61521.

After these investigations I think I may conclude that the law of change of refrangibility of light is perfectly correct in the general form in which it was given by Stokes.—*Comptes Rendus de l'Académie des Sciences*, June 9, 1879.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

SEPTEMBER 1879.

XXII. *Chemical Affinity.* By M. M. PATTISON MUIR, *Prælector in Chemistry, Gonville and Caius College, Cambridge**.

IN the year 1780 Bergmann formulated a general theory of chemical affinity. The leading points in Bergmann's theory were these. The affinity between two bodies is independent of the masses of these bodies which may be brought into mutual contact; under like condition the value of this affinity is constant. The relative affinity-values of various substances may be empirically represented by the amounts of these bodies which mutually combine together: thus, if an acid combine with a series of bases to form neutral salts, the affinity of the acid is greatest for that base the greatest amount of which is taken up by the acid. Conversely, a base has the greatest affinity for that acid which combines with it in largest quantity.

In 1803 Berthollet published his great work *Statique Chimique*, in which he sought to show that the chemical action of one substance upon a second substance is proportional to the mass of the first and its affinity for the second. The product of these two quantities is called by Berthollet the "chemical mass" of the substance. Complete decomposition of one compound by another is never brought about, according to Berthollet, by chemical affinity alone; this force must always be aided by what the French chemist called cohesion and elasticity.

Bergmann considered only the affinity of one body for

* Communicated by the Author.

Phil. Mag. S. 5. Vol. 8. No. 48. *Sept.* 1879.

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another; Berthollet considered also the masses of the acting bodies and the physical actions which aided the purely chemical changes. A substance with a small affinity for either of the constituents of another substance was nevertheless, according to Berthollet, capable of decomposing the other, provided the mass of the first substance was large compared with that of the second. The affinity of an acid is in this view greatest for that base with which it combines in smallest quantity, because, if the affinity be large, the mass must be small as compared with the mass of another base which has a small affinity for the given acid; otherwise equal amounts of chemical work would not be done in forming each of the neutral salts.

The theories of Bergmann and Berthollet were antagonistic. Since the year 1803 chemists have now inclined to one, now to another. Modifications have been made in each; but no new important theory has been advanced.

The most important contributions made within recent years towards the final solution of the problem of chemical affinity are contained in two papers by Guldberg and Waage, and three papers by W. Ostwald.

The object of this paper is to give an account of the results obtained by these naturalists.

The first paper by the two Christiania Professors was published in 1867, and is entitled "*Études sur les affinités chimiques*;" the second paper by the same authors appeared in the March number of the present year of the *Journal für praktische Chemie*. Ostwald's papers, entitled "*Volumchemischen Studien*," are to be found in *Pogg. Ann. Ergänzg.* Bd. viii. p. 154, and in *Journ. prakt. Chem.* [2] xvi. 385, and xviii. 328.

Guldberg and Waage do not attempt to formulate a general theory of chemical action; they confine themselves to a consideration of the *action of mass* in chemical changes. The explanation which they give of this action is singularly simple, and so exact as to allow of mathematical deductions being made, which are shown to hold good by the results of experimental researches.

Ostwald considers the measurement of affinity as exerted between acids and bases in solution combining together to form salts which are soluble under the experimental conditions. Berthollet's statement, "*Toute substance qui tend à entrer en combinaison, agit en raison de son affinité et de sa quantité*" (*Statique Chimique*, p. 2), has been extended and rendered more exact by the researches of Guldberg and Waage. These naturalists have given a definite meaning, independent

of hypothesis, to the expression "coefficient of affinity." They thus express themselves:—

"Let two bodies, A and B, be converted, by double substitution, into A' and B', and let A' and B' be again reconverted, under the same conditions, into A and B, neither of those changes will be complete. At the close of the reaction there are present four bodies A, B, A', and B'; and the force which caused the formation of A' and B' is held in equilibrium by the force which caused the formation of A and B. The force causing the formation of A' and B' increases proportionately to the coefficient of affinity of the reaction, but is also dependent upon the quantities of A and B: we have found that this force is proportional to the product of the active masses of A and B. If the latter be p and q respectively, and the coefficient of affinity be k , then the force is represented by

$$k \cdot p \cdot q.$$

This expression also evidently represents the amounts of A and B transformed in unit time into A' and B'. . . . If the active masses of A' and B' be p' and q' respectively, and the affinity-coefficient of the reaction $A' + B' = A + B$ be equal to k' , then the force which tends to bring about the reformation of A and B is

$$k'p'q'.$$

This force being held in equilibrium by the first, we get the equation

$$kpq = k'p'q'.$$

"By experimentally determining p, q, p', q' , the proportion $k : k'$ can be calculated; and from this the result of the reaction for each initial condition can be determined."

Active mass of a body is defined as the amount of that substance in unit volume of the chemical system which undergoes change. The coefficient of affinity is dependent upon the chemical nature of A and B and upon the temperature.

The equation expressing the conditions of equilibrium ($kpq = k'p'q'$) holds good only when the action of secondary forces, *i. e.* forces whose action is to be traced to the presence of compounds other than A, B, A', or B' is overlooked.

"If the number of molecules of A, B, A', and B' before the reaction be represented by P, Q, P', and Q', and if x be the number of molecules of A and B transformed into A' and B', then, supposing the total volume to remain constant during the reaction, we have

$$p = \frac{P-x}{V}, \quad q = \frac{Q-x}{V}, \quad p' = \frac{P'+x}{V}, \quad q' = \frac{Q'+x}{V};$$

and by substituting these values in the equation of equilibrium and multiplying by V^2 , we get the general equation

$$(P-x)(Q-x) = \frac{k'}{k}(P'+x)(Q'+x).''$$

Those reactions which consist of two parts, the direct and the reverse chemical change, are most favourable for the study of the influence of mass. As examples of this class of changes may be cited:—the oxidation of a metal by water-gas, and the reduction of the metallic oxide so produced by hydrogen; the dissociation of a substance $A B$ under conditions such that $A B$ and A and B (the products of the dissociation) are simultaneously present in the chemical system; the division of two acids between two bases so as to produce a soluble and an insoluble salt; and the mutual decomposition of two soluble compounds with the production of substances themselves soluble under the experimental conditions.

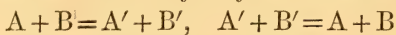
In their earlier paper Guldberg and Waage give the results of a long and elaborate series of experiments upon the action of carbonates of the alkalis on barium sulphate, and of alkali sulphates upon barium carbonate; they also consider the results obtained by Berthelot and St. Giles in their examination of the reaction between alcohol and acetic acid, which results in the production of acetic ether and water. From these, and other quantitative results, the law of mass-action already stated was deduced.

In their later paper the same naturalists detail the results of their examination of many different reactions, and show that in each case the law of mass-action holds good.

Guldberg and Waage recognize two main groups of chemical forces:—the true *forces of affinity*, which bring about the formation of new chemical substances; and the *secondary forces*, the action of which is to be traced to the presence of foreign bodies, *i. e.* bodies other than those directly undergoing chemical change.

Guldberg and Waage do not think that a complete account of a chemical change involving simultaneous series of decompositions and recompositions can be given, if regard be paid merely to the forces of attraction at work between the substances, or the constituents of the substances, upon which the change is effected; they deem it necessary to consider the molecular and atomic movements of these substances.

The equilibrium attained by a system in which the changes



have been allowed to proceed is regarded by them as by no

means the equilibrium of a system the parts of which are at rest.

So long as equal amounts of A' and B', and of A and B, are formed in unit time, so long is the equilibrium of the system maintained.

Let it be supposed that the molecule A consist of the atoms (or molecules) α and γ , these atoms will continually perform their proper vibrations within the molecule A. At certain positions of α and γ we may suppose that the force acting between these two atoms is very small. Let another molecule B be composed of the atoms β and δ , these atoms will also perform vibrations within B; A and B will also each be possessed of its own proper motion. If A and B approach each other at the moment when α and γ and β and δ are respectively at those positions at which the force between them is very small, then a chemical force may act between β and γ and between α and δ which shall result in the production of two new molecules A' and B'. The reverse action, resulting in the production of A and B, may be supposed to take place in a manner similar to that whereby A' and B' have been formed.

A similar view may be taken of the formation of an *addition compound*. The compound molecule ABC may be split up, under certain conditions, into A, B, and C, while by the simultaneous recombination of the molecules A, B, and C new compound molecules of the form ABC may be produced.

In order to determine the velocity of formation of the new substances in such cases as the preceding, let p and q represent the number of the molecules of A and B in unit volume; further, let a be the number of p molecules which are so conditioned that by coming into contact with the molecules B they suffer decomposition, and let b represent the number of q molecules which are similarly conditioned with regard to A. In unit volume there are therefore ap molecules of the substance A, and bq molecules of the substance B, which by their mutual meeting together are capable of being transformed into new substances. The frequency of the meeting of undecomposed molecules will be represented by the product $ap \cdot bq$, and the velocity of formation of the new substances by the equation

$$\phi \cdot apbq = k p q,$$

where $k = \phi ab$.

If it be required that three substances A, B, and C suffer mutual decomposition in order that a new compound be produced, and if the number of molecules of each in unit volume be p , q , and r respectively, then, expressing the proper

coefficient of the substances by a , b , and c , the expression for the velocity of formation of the new compound becomes

$$\phi \cdot apbqcr = kpqr,$$

where k is again taken as representing the product of the coefficients.

The velocity of formation of an addition compound consisting of α molecules of A, β molecules of B, and γ molecules of C, is expressed as follows:—

$$\begin{aligned}\phi \cdot apap \dots bqbq \dots crcr \\ &= \phi \cdot a^\alpha p^\alpha b^\beta q^\beta c^\gamma r^\gamma \\ &= kp^\alpha q^\beta r^\gamma,\end{aligned}$$

where k expresses the product of all the coefficients.

The velocity-coefficient, as also the coefficients a , b , c , must be considered as dependent not only upon the nature of the substances in the system, but also upon the temperature; the nature of this dependence can only be discovered by experiment.

If the velocity of formation of the new substances be thus found, it is only necessary to equalize the velocity of the two opposing reactions in order to arrive at the conditions of equilibrium of the system.

The absolute velocity with which the chemical change proceeds is evidently equal to the difference between the velocities of the two opposing reactions.

It is evident that this development of the theory of mass-action supposes that the influence of *secondary forces* (p. 183) is overlooked. It is probable that the action of these forces may be disregarded when very dilute solutions of the reacting substances are employed.

From their general treatment of the subject of chemical equilibrium, Guldberg and Waage have been led to regard those processes as complete in which chemical reactions simultaneously proceed in two opposite directions. On the other hand, when but one of these opposing changes is accomplished, they regard the process as incomplete. By withdrawal from the sphere of action of one or more of the products of the first part of the process, or by the occurrence of secondary reactions, or by the maintenance of the temperature within certain limits, or by the assumption, by the affinity-coefficient, of a value such that a condition of equilibrium is attained when extremely small quantities of the reacting bodies are present—by these circumstances the occurrence of the reverse action, and hence the completion of the chemical process, may be prevented.

The application of their general theory of mass-action is considered by Guldberg and Waage with reference to various classes of chemical systems.

1. *Systems consisting of four Soluble Substances.*

One pair of bodies A and B₁ is decomposed into another pair A₁ and B in accordance with the equation

$$A + B_1 = A_1 + B.$$

If the amounts of the individual substances (expressed for the sake of simplicity in equivalent units) which are present when equilibrium is established be represented by p, p_1, q, q_1 , and if V be the total volume of the solution, then the active masses are represented by

$$\frac{p}{V}, \frac{p_1}{V}, \frac{q}{V}, \text{ and } \frac{q_1}{V},$$

the velocity with which the formation of A and B proceeds
by

$$\phi \cdot a \frac{p}{V} \cdot b_1 \frac{q_1}{V},$$

and the velocity with which the formation of A_1 and B_1 proceeds by

$$\phi \frac{a_1 p_1}{V} \cdot \frac{b q}{V}.$$

Deducing the condition of equilibrium, and taking k as $\frac{a}{b}$, and $k_1 = \frac{a_1}{b_1}$, then

$$k \frac{p}{q} = k_1 \frac{p_1}{q_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If the original quantities of the four substances be P, P₁, Q, and Q₁ respectively, and if we suppose that equilibrium ensues when a quantity ξ of the substances A and B is decomposed, then

$$\begin{aligned} p &= P - \xi, & p_1 &= P_1 + \xi, \\ q &= Q + \xi \text{ and } q_1 &= Q - \xi. \end{aligned}$$

Putting these values into equation (1) and taking $\frac{k}{k_1} = \kappa$, we obtain

$$\xi = \frac{\kappa(P + Q_1) + P_1 + Q}{2(\kappa - 1)} \mp \sqrt{\left\{ \frac{\kappa(P + Q_1) + P_1 + Q}{2(\kappa - 1)} \right\}^2 + \frac{P_1 Q - \kappa Q_1}{\kappa - 1}}; \quad (2)$$

when $\kappa > 1$, the minus sign is employed, and the positive sign when $\kappa < 1$. The value of ξ is positive when $\kappa \frac{P}{Q} > \frac{P_1}{Q_1}$.

If it be desired to study the influence of time upon the course of the reaction, it becomes necessary to introduce the absolute velocity. If x be the amounts of A and B₁ decomposed to A₁ and B in time t , then the amounts decomposed in an infinitely short time dt is dx , and the velocity is $\frac{dx}{dt}$.

By further development, and by integration, the following formula is found:—

$$\log \text{nat.} \left(\frac{\xi}{\xi - x} \cdot \frac{h - x}{h} \right) = \phi \frac{a_1 b}{V^2} (\kappa - 1)(h - \xi) \cdot t, \quad (3)$$

where

$$h = \frac{\kappa(P + Q_1) + P_1 + Q}{\kappa - 1} - \xi.$$

From this equation it would appear that x attains the value ξ only after an infinite time; but the function in equation (3) is of such a character that the difference between x and ξ becomes so small after a comparatively short time, that it may be disregarded in an experiment.

The first special cases considered by Guldberg and Waage as illustrative of the systems now under examination is that in which A = acetic acid, B = water, A₁ = ether, B₁ = alcohol. From the experiments of Berthelot and St. Giles it may be shown that $\frac{k}{k_1} = 4$.

The values given in the following Table for ξ have been calculated by the aid of equation (2). The secondary reactions in this special case exert a considerable influence upon the primary chemical change.

Initial quantities of				Quantity of acetic acid decomposed.	
Acetic acid. P.	Alcohol. Q ₁	Ether. P ₁	Water. Q.	Observed. ξ.	Calculated. ξ.
1	1	0	0	0.665	0.667
1	2	0	0	0.828	0.845
1	4	0	0	0.902	0.930
2	1	0	0	0.858	0.845
1	1	1.6	0	0.521	0.492
1	1	0	3	0.407	0.409
1	1	0	23	0.116	0.131
1	2	0	98	0.073	0.073

The authors also consider the special case in which A =ferric chloride, B =hydrochloric acid, A_1 =ferric oxide, and B_1 =water; and also that in which A =nitric acid, B =sodium nitrate, A_1 =sulphuric acid, and B_1 =sodium sulphate. The experimental data for the first case were accumulated by G. Wiedemann (*Pogg. Ann.* 1878), and those for the second case are taken from the researches of Thomsen.

It is evident from equation (1) that the proportion $k_1:k$ is determined by experiment, but not the actual value of k_1 and k . By carrying out analogous experiments with new systems A , B , A_2 , B_2 , &c., it is easy to determine the new proportion k_2 &c.: k . If, now, k be taken as $=1$, the relative values of k_1 , k_2 , &c. are obtained in reference to the pair AB . A Table may be thus drawn up by the aid of which the conditions of equilibrium for those systems which any two pairs of bodies in the Table are capable of forming among themselves may be calculated.

Thus, if it be wished to calculate the conditions of equilibrium for the system A_1 , A_2 , B_1 , B_2 , the equation is

$$k_1 \frac{p_1}{q_1} = k_2 \frac{p_2}{q_2};$$

and the values of k_1 and k_2 are found from the Table.

The three following Tables have been drawn up from data taken from Thomsen's researches.

TABLE I.

Relative values of k .

A.		B.	k .
Hydrochloric acid.	HCl	NaCl	1
Nitric acid.	HNO ₃	NaNO ₃	1
Sulphuric acid.	$\frac{1}{2}(\text{H}_2 \text{SO}_4)$	$\frac{1}{2}(\text{Na}_2 \text{SO}_4)$	0·25
Oxalic acid.	$\frac{1}{2}(\text{H}_2 \text{C}_2 \text{O}_4)$	$\frac{1}{2}(\text{Na}_2 \text{C}_2 \text{O}_4)$	0·0676
Phosphoric acid.	H ₃ PO ₄	Na H ₂ PO ₄	0·0625
Tartaric acid.	$\frac{1}{2}(\text{C}_4 \text{H}_6 \text{O}_6)$	$\frac{1}{2}(\text{C}_4 \text{H}_4 \text{Na}_2 \text{O}_6)$	0·0025
Citric acid.	$\frac{1}{3}(\text{C}_6 \text{H}_8 \text{O}_7)$	$\frac{1}{3}(\text{C}_6 \text{H}_5 \text{Na}_3 \text{O}_7)$	0·0025
Acetic acid.	C ₂ H ₄ O ₂	C ₂ H ₃ NaO ₂	0·0009
Boric acid.	HBO ₂	NaBO ₂	0·0001

TABLE II.

A.	B.	k .
Hydrochloric acid.	Metallic chloride.	1
Sulphuric acid.	Metallic sulphate.	0·25

The metal may be potassium, sodium, or ammonium.

TABLE III.

A.	B.	k.
Hydrochloric acid.	Metallic chloride.	1
Sulphuric acid.	Metallic sulphate.	0.5

The metal may be magnesium, iron, manganese, nickel, cobalt, or copper.

TABLE IV.

A.	B.	k.
R''Cl.	$\frac{1}{2}(R_2''SO_4)$.	1
R'''Cl.	$\frac{1}{2}(R_2'''SO_4)$.	2

R'' = metal of Table II. R''' = metal of Table III.

2. Systems consisting of two Insoluble and two Soluble Substances.

In the case of insoluble compounds the active mass of the substance does not necessarily decrease in the same proportion as the total mass of the substance present. Increase of the absolute amount of an insoluble substance does not increase the active mass of that substance to any sensible extent. Thus, 1 grm. of an insoluble salt in 100 cub. centims. of liquid produced the same effect as 2 grms. of the salt.

Supposing, then, that the masses of the insoluble substances are constant during the experiments, these masses enter the equations as unknown constant quantities, the value of which may be determined from the experimental data, either independently or along with other unknown constants. Under these conditions, the proportion between the amounts of the two soluble substances is always the same when the system is in equilibrium.

The value of ξ is determined for the systems now under consideration by aid of the formula

$$\xi = \frac{P_1 - c_1 Q_1}{1 + c_1}, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where

$$c_1 = \frac{p_1}{q_1}$$

(p_1 and q_1 = masses of A_1 and B_1 , the soluble substances present). If the element of time be introduced, equation (4) becomes

$$\log \text{nat.} \left(\frac{\xi}{\xi - x} \right) = \phi \frac{a_1 b q (1 + c_1)}{V^2} \cdot t, \quad . \quad . \quad . \quad (5)$$

x = amount of substance A_1 transformed into B_1 in time t .

3. Systems consisting of an indefinite Number of Soluble Substances.

When several chemical reactions proceed simultaneously in the same solution, the velocity of each individual reaction is independent of the other reactions.

The conditions of equilibrium for such a system are expressed by the formula

$$k \frac{P}{q} = k_1 \frac{P_1}{q_1} = k_2 \frac{P_2}{q_2} = \dots k_n \frac{P_n}{q_n}.$$

Putting $\frac{P}{q} = z$, it may be shown that

$$q = \frac{P + Q}{1 + z}, \text{ and } q_1 = \frac{P_1 + Q_1}{1 + \frac{k}{k_1} z} \text{ \&c.}$$

The general equation is deduced

$$\frac{P + Q}{1 + z} + \frac{P_1 + Q_1}{1 + \frac{k}{k_1} z} + \dots + \frac{P_n + Q_n}{1 + \frac{k}{k_n} z} = Q + Q_1 + \dots Q_n. \quad (6)$$

The value of z can be found by aid of this equation, and from that the value of $q, q_1, \dots q_n$, and finally the value of $p, p_1, \dots p_n$.

Several illustrations are given of the application of this formula; but no experimental data have as yet been accumulated.

As an example of the method of solving the problem presented by a system consisting of many soluble substances, the question is discussed:—How would one equivalent of soda divide itself between 1 equivalent of hydrochloric acid, 1 equivalent of sulphuric acid, and 1 equivalent of oxalic acid?

Let the original system consist of 1 hydrochloric acid + 1 sulphuric acid + 1 sodium sulphate; and let A = hydrochloric acid, A_1 = sulphuric acid, A_2 = oxalic acid, B = sodium chloride, B_1 = sodium sulphate, and B_2 = sodium oxalate; then

$$\begin{aligned} P &= 1, & P_1 &= 1, & P_2 &= 0, \\ Q &= 0, & Q_1 &= 0, & Q_2 &= 1. \end{aligned}$$

From Table I. (p. 189) the following values are found:—

$$k = 1, \quad k_1 = 0.25, \quad k_2 = 0.0676.$$

Equation (6) then gives

$$\frac{1}{1 + z} + \frac{1}{1 + 4z} + \frac{1}{1 + 14.8z} = 1.$$

By repeated trials of various values for z , the approximate value of that proportion is found to be 0.62. Hence the following equivalent proportions are determined for the con-

ditions of equilibrium of the system :—

Sodium chloride $q = 0.62$	Hydrochloric acid $p = 0.38$
Sodium sulphate $q_1 = 0.29$	Sulphuric acid ... $p_1 = 0.71$
Sodium oxalate $q_2 = 0.10$	Oxalic acid..... $p_2 = 0.90$

Of the remaining systems considered the more important are :—

4. *Systems consisting of gaseous substances arising from the dissociation of a solid* ; and 5. *Systems consisting entirely of gaseous substances.*

If a gas M be an addition compound of the form $\alpha A + \beta B + \gamma C$, then, by dissociation, one molecule of this gas will be resolved into α molecules of A , β molecules of B , and γ molecules of C . If, further, p , q , and r be the amounts of the different components in unit volume, and if the most general case be considered, in which, besides the amounts present of p , q , r (the dissociated constituents of M), an amount P of the original compound is also present, then the equation of equilibrium is deduced,

$$p^\alpha q^\beta r^\gamma = \phi[P + F(t)]. \quad . \quad . \quad . \quad (7)$$

This equation is also applicable to cases in which indifferent gases, other than A , B , or C are present. The equation is developed and applied by the authors especially to the results of experiments upon the dissociation of N_2O_4 into NO_2 where the product of decomposition of the original compound is alone present, and to the dissociation of HI in presence of either of the products of decomposition (see Deville and Troost, *Compt. Rend.* 1875, and A. Naumann, *Berl. Ber.* x. 2045 ; see also Lemoine, *Ann. Chim. Phys.* [5] xii. 145).

In his earliest paper (Pogg. *Ergzsb.* viii.) Ostwald describes the experimental method which he has adopted, viz. measurement of the change in specific volume of a liquid ; and shows that it is possible by this method to measure small amounts of chemical change occurring between those substances solutions of which are mixed to form the liquid in question. Thus, if the specific volumes of normal solutions of potassium sulphate and of nitric acid be determined, if these solutions be mixed, and if the specific volume of the mixed liquid be then determined, it is found that a certain amount of expansion has taken place, and that this expansion measures the amount of chemical action which has occurred*. Ostwald

* K. Hofmann (Pogg. *Ann.* cxxxiii. 575, 1868) seems to have been the first who attempted to deduce measurements of chemical change from determinations of sp. vol. and of coefficients of refraction. His attempt, however, was not altogether successful.

shows that his method yields results identical with those obtained by Thomsen by means of the thermochemical method.

Ostwald has also determined the coefficients of refraction of a series of normal solutions of acids and of bases, and the coefficients of refraction of the liquids produced by mixing these; and in this way he has arrived at results concerning the chemical action between the acids and bases in question, which corroborate those obtained by the specific-volume method.

When aqueous solutions of acids and bases are mixed in equivalent quantities, the volume of the product is different from that of the sum of the volumes of the constituents. This change of volume varies with the acid, the base, the temperature, and the degree of concentration. The two latter conditions being kept constant, a value is obtained for each combination of acid and base. The normal temperature employed was 20° ; the normal concentration 1 equivalent (in grams) of the acid or base, in 1000 grams of the solution.

In the general reaction $A + B = A' + B'$, let A and A' be the acids, let B' be the neutral salt of the acid A , and B the neutral salt of the acid A' *with the same base*; then the coefficient of affinity may be defined, for this reaction, as the proportion in which the base divides itself between the two acids when the three substances mutually react in equivalent quantities. The amount of base taken up by each acid is a measure of the affinity of the acid for the base; the coefficient of affinity expresses the relation between these affinities, or, in other words, the *relative affinity* of the acids for the base. The relative affinity of the acids is a function of their absolute affinity, and must be studied under those conditions which influence the latter. These conditions are nature of the base, temperature, and perhaps pressure. The latter was constant throughout the investigations to be described.

Let the changes of volume occurring when the acids A and A' combine with the same base C be expressed by v and v' . Let the acid A withdraw a portion x of the base, A' will withdraw $1 - x$. Then the resulting change of volume v_0 will be

$$v_0 = xv + (1 - x)v',$$

supposing, that is, that no change of volume is brought about by secondary reactions;

$$\therefore x = \frac{v_0 - v'}{v - v'}.$$

The greater the difference $(v - v')$ between the changes of volume occurring in the neutralization of each of the acids

separately, the more accurate will the determination of the division of the same base between these acids become.

In the reaction between the salt $A'C$ and the acid A , whereby x parts of $A'C$ are decomposed with formation of x parts of AC , the changes of volume are $v'x$ and vx (the value of $v'x$ is taken as negative because it represents a decomposition), and the observed volume-change in the liquid containing the reacting bodies is $v_1 = vx - v'x + \xi$, where ξ expresses the sum of the secondary reactions.

Similarly it is shown that the observed volume-change in the reverse action, viz. decomposition of x parts of AC by A' , is

$$v_2 = v'(1-x) - v(1-x) + \xi.$$

From these equations the value of x is deduced,

$$x = \frac{v_1 - \xi}{v_1 - v_2}, \quad 1 - x = -\frac{v_2 - \xi}{v_1 - v_2}.$$

These equations are applied by Ostwald to the experimental results which he has obtained.

The relative affinities of sulphuric, hydrochloric, and nitric acids for the alkalis, magnesia, zinc oxide, and copper oxide are considered by Ostwald in his second paper.

In the following tables v_1 represents the volume-change accompanying the action of nitric or hydrochloric acid on the sulphates of the bases mentioned, v_2 the volume-change accompanying the action of sulphuric acid upon the nitrates or chlorides of the same bases, $v - v'$ represents the calculated differences of the volume-changes caused by neutralization.

TABLE I.

Influence of Base.

Nitric and Sulphuric Acids.

Base.	v_1 .	v_2 .	$v_1 - v_2$.	$v - v'$.
Potash	+14.00	-2.38	+16.38	+16.34
Soda	+13.77	-2.73	+16.50	+16.50
Ammonia	+11.64	-2.70	+14.34	+14.49
Magnesia	+10.58	-3.06	+13.64	+13.35
Zinc oxide	+ 8.86	-3.11	+11.97	+12.54
Copper oxide ...	+ 7.85	-3.42	+11.27	+10.69

TABLE II.

Influence of Base.

Hydrochloric and Sulphuric Acids.

Base.	v_1 .	v_2 .	$v_1 - v_2$.	$v - v'$.
Potash	+13.08	-2.09	+15.17	+15.56
Soda	+13.00	-2.52	+15.52	+15.50
Ammonia	+11.45	-2.78	+14.28	+14.16
Magnesia	+10.47	-3.05	+13.52	+13.60
Zinc oxide.....	+ 9.08	-3.32	+12.40	+12.87
Copper oxide ...	+ 8.06	-3.49	+11.55	+11.30

TABLE III.

Volume-changes accompanying the action of Sulphuric Acid upon Sulphates : $\text{RSO}_4 + n\text{H}_2\text{SO}_4$.

$n =$	$\frac{1}{2}$	1.	2.	4.	8.
Potash	+4.55	+7.09	+9.25	+9.80	+10.58
Soda	+4.17	+6.32	+8.33	+8.98	+ 9.06
Ammonia	+2.77	+5.14	+6.78	+7.85	+ 7.93
Magnesia	+2.66	+4.11	+5.54	+6.44	+ 6.77
Zinc oxide.....	+1.83	+2.89	+3.71	+4.51	+ 5.01
Copper oxide..	+1.37	+2.25	+2.93	+3.02	+ 3.26

TABLE IV.

Volume-changes accompanying the action of Nitric Acid on Nitrates, and Hydrochloric Acid on Chlorides.

	$\text{RN}_2\text{O}_6 + \text{H}_2\text{N}_2\text{O}_6$.	$\text{RCl}_2 + \text{H}_2\text{Cl}_2$.
Magnesia	+0.07	+0.16
Zinc oxide.....	+0.52	+0.92
Copper oxide	+0.34	+0.66

In order to find from these data the value of x in the equation

$$x = \frac{v_1 - \xi}{v_1 - v_2},$$

it is necessary to proceed by systematic trials, inasmuch as ξ is dependent upon x , but the form of this dependence is unknown*.

* ξ is composed of the volume-changes accompanying the reactions $(1-x)\text{RSO}_4 + x\text{H}_2\text{SO}_4$, and $x\text{RN}_2\text{O}_6 + (1-x)\text{H}_2\text{N}_2\text{O}_6$ (or $x\text{RCl}_2 + [1-x]\text{H}_2\text{Cl}_2$); in the case of the alkalis the last member of this group of changes = 0.

By proceeding thus the following Table is prepared. In this table column I. expresses the relative affinity $\frac{\text{H}_2\text{N}_2\text{O}_6}{\text{H}_2\text{SO}_4}$; column II. the relative affinity $\frac{\text{H}_2\text{Cl}_2}{\text{H}_2\text{SO}_4}$; and column III. the relative affinity $\frac{\text{H}_2\text{Cl}_2}{\text{H}_2\text{N}_2\text{O}_6}$.

The relative affinities of columns I. and II. are calculated from the equation $k = \frac{x}{1-x}$, and those of column III. by dividing II. into I.

(On account of the smallness of the difference $v_1 - v_2$, the affinities of Column III. could not be directly determined by the volumetric method.)

TABLE V.

Influence of bases upon Relative Affinities.

Base.	I. $\frac{\text{H}_2\text{N}_2\text{O}_6}{\text{H}_2\text{SO}_4}$.	II. $\frac{\text{H}_2\text{Cl}_2}{\text{H}_2\text{SO}_4}$.	III. $\frac{\text{H}_2\text{Cl}_2}{\text{H}_2\text{N}_2\text{O}_6}$.
Potash	0·667 = 2·00 0·333	0·659 = 1·94 0·341	1·94 = 0·97 2·00
Soda	0·667 = 2·00 0·333	0·657 = 1·92 0·343	1·92 = 0·96 2·00
Ammonia	0·652 = 1·88 0·348	0·644 = 1·81 0·356	1·81 = 0·96 1·88
Magnesia	0·638 = 1·76 0·362	0·635 = 1·74 0·365	1·74 = 0·99 1·76
Zinc oxide.....	0·617 = 1·61 0·383	0·605 = 1·53 0·395	1·53 = 0·95 1·61
Copper oxide ...	0·591 = 1·44 0·409	0·584 = 1·40 0·416	1·40 = 0·97 1·44

From these results it appears that the affinity-proportion $\text{H}_2\text{SO}_4 : \text{H}_2\text{N}_2\text{O}_6$ and $\text{H}_2\text{SO}_4 : \text{H}_2\text{Cl}_2$ is dependent upon the nature of the base, but that $\text{H}_2\text{Cl}_2 : \text{H}_2\text{N}_2\text{O}_6$ is independent of the base. From considerations regarding the volume-changes accompanying the action of sulphuric acid upon sulphates, Ostwald shows that the whole mass of this acid is not to be regarded in determining the affinity of the acid towards bases, but only that part which is not converted into acid sulphate. Hence he concludes that probably the true relative affinity of sulphuric acid, as that of nitric and hydrochloric acids, is independent of the nature of the base neutralized.

The influence of temperature upon the relative affinities of the acids is set forth in the following Tables:—

TABLE VI.

Influence of Temperature.

Nitric and Sulphuric Acids.

Temp.	v_1 .	v_2 .	$v_1 - v_2$.
°			
0	+11·49	-3·39	+14·88
20	+13·77	-2·73	+16·50
40	+15·95	-2·15	+18·10
60	+18·16	-1·29	+19·45
<i>Hydrochloric and Sulphuric Acids.</i>			
°			
0	+10·79	-2·97	+13·76
20	+12·99	-2·52	+15·51
40	+15·36	-1·89	+17·25
60	+17·77	-1·04	+18·81
$\text{Na}_2\text{SO}_4 + n\text{H}_2\text{SO}_4$.			
Temp.	$n=1$.	$n=2$.	
°			
0	+ 4·11	+ 5·46	
20	+ 6·32	+ 8·33	
40	+ 9·15	+11·62	
60	+11·74	+14·94	

TABLE VII.

Influence of Temperature upon Relative Affinities.

Temp.	I. $\frac{\text{H}_2\text{N}_2\text{O}_6}{\text{H}_2\text{SO}_4}$.	II. $\frac{\text{H}_2\text{Cl}_2}{\text{H}_2\text{SO}_4}$.	III. $\frac{\text{H}_2\text{Cl}_2}{\text{H}_2\text{N}_2\text{O}_6}$.
°			
0	0·655 = 1·90	0·659 = 1·93	1·93 = 1·02
	0·345	0·341	1·90
20	0·667 = 2·00	0·657 = 1·92	1·92 = 0·96
	0·333	0·343	2·00
40	0·669 = 2·02	0·666 = 1·99	1·99 = 0·98
	0·331	0·334	2·02
60	0·703 = 2·37	0·703 = 2·37	2·37 = 1·00
	0·297	0·297	2·37

It is evident from these Tables that the relative affinities of hydrochloric and nitric acids are independent of the temperature.

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ture, while that of sulphuric acid varies with the temperature, but in a manner inversely as the binding of that acid by neutral sulphate (see Table VI.).

From these results Ostwald concludes that most probably *the relative affinity of the acids is a constant number independent of base, and independent of temperature*; he believes that this generalization holds good for all the acids.

If the absolute affinity of an acid A for a base C be represented as a function of both, the above generalization may be expressed in the form

$$\frac{f(A_1C)}{f(A_1'C)} = \frac{f(A_1C')}{f(A_1'C')};$$

and from this it follows that

$$\frac{f(A_1C)}{f(A_1C')} = \frac{f(A_1'C)}{f(A_1'C')};$$

i. e. *the relative affinities of the bases are independent of the particular acid used for neutralization.*

From these relations Ostwald deduces the result that the function $f(A_1C)$ is a product of two factors, one containing only A_1 and one only C :

$$f(A_1C) = \phi(A) \cdot \psi(C);$$

or, *the affinity between an acid and a base is a product of the specific affinity-constants of the acid and base.*

If the regularities observed for the special cases now described be regarded as expressing a true generalization, the affinities which come into play in the formation of salts may be simply expressed in a Table such as that given below.

The relative affinities of all bases being determined in reference to one acid, and those of all acids in reference to one base, and the affinity of this acid to this base being taken as unit, the values so obtained may be arranged as follows:—

	$\phi(A).$	$\phi(A').$	$\phi(A'').$	$\phi(A''').$
$\psi(C) \dots\dots$	.	.	.	.
$\psi(C') \dots\dots$	.	.	.	.
$\psi(C'') \dots$	.	.	.	.
$\psi(C''') \dots$	.	.	.	.

The product of the expression at the head of one of the horizontal columns, $\psi(C)$, with that at the head of one of the vertical column, $\phi(A)$, gives the magnitude of the affinity

exerted between the corresponding base and acid. By developing the Table in a third direction in space, expression might be given to the influence of temperature upon the absolute affinity (which influence is yet to be determined). The fact that temperature is without influence upon the relative affinity means only that the influence of temperature upon the absolute affinity is the same for all compounds of acid and base.

In his third paper Ostwald extends his researches upon the neutralization of acids by bases. The following Tables contain the principal results.

The + sign denotes expansion of volume, the — sign contraction; the numbers in brackets [] express the differences between the sums of the coefficients of refraction of the acids and bases, determined separately, and the coefficient of refraction of the solutions formed on mixing these. The + sign indicates an increase, the — sign a decrease in the coefficient of refraction. The determinations of refraction-coefficients were made at 20°, the sodium light being employed.

TABLE VIII.
Neutralization of Monobasic Acids.

	Potash.	Soda.	Ammonia.
Nitric acid.....	+20·046 [—897]	+19·770 [—886]	— 6·441 [+139]
Hydrochloric acid ...	+19·521 [—882]	+19·238 [—879]	— 6·572 [+148]
Hydrobromic acid ...	+19·626 [—916]	+19·336 [—907]	— 6·567 [+117]
Hydriodic acid.....	+19·799 [—938]	+19·335 [—923]	— 6·442 [+100]
Formic acid	+12·361 [—484]	+12·153 [—478]	—13·593 [+536]
Acetic acid	+ 9·522 [—363]	+ 9·287 [—354]	—16·261 [+646]
Monochloracetic acid..	+10·855 [—425]	+10·628 [—412]	—15·087 [+603]
Dichloracetic acid ...	+12·946 [—552]	+12·702 [—544]	—12·975 [+473]
Trichloracetic acid ...	+17·357 [—774]	+17·067 [—774]	— 8·665 [+264]
Propionic acid	+ 7·830 [—318]	+ 7·679 [—306]	—17·822 [+699]
Butyric acid	+ 6·984 [—279]	+ 6·844 [—269]	—18·633 [+730]
Isobutyric acid	+ 6·301 [—254]	+ 6·174 [—248]	—19·270 [+758]
Glycollic acid	+ 9·616 [—]	+ 9·517 [—]	—16·496 [—]
Lactic acid	+ 8·267 [—353]	+ 8·133 [—344]	—17·739 [+661]

If the values which the same acids give with different bases be considered, it is found that they are always positive [negative] for potash and soda, and always negative [positive] for ammonia. The two following Tables contain the differences between the numbers obtained by neutralizing the same acid with two different bases, and the differences between the numbers obtained by neutralizing different acids with the same base.

From these results it is seen that, *the differences between the numbers found for any two bases are nearly constant through-*

out the series of acids. Further, the differences between the numbers found for any two acids are nearly constant for the three bases.

In Table X. the differences are those between the number found for isobutyric acid (which had the smallest observed value) and that for each of the other acids.

In Table IX. the vertical columns represent the constant differences, in Table X. the horizontal lines.

TABLE IX.
Differences referred to Acids.

	Potash-Ammonia.	Soda-Ammonia.	Potash-Soda.
Nitric acid	26.487 [1036]	26.211 [1027]	0.276 [9]
Hydrochloric acid ...	26.093 [1030]	25.810 [1027]	0.283 [3]
Hydrobromic acid ..	26.191 [1033]	25.901 [1024]	0.290 [9]
Hydriodic acid.....	26.241 [1038]	25.977 [1023]	0.264 [15]
Formic acid	25.954 [1020]	25.746 [1014]	0.202 [7]
Acetic acid	25.783 [1009]	25.548 [1000]	0.235 [8]
Monochloracetic acid..	25.942 [1028]	25.715 [1015]	0.227 [13]
Dichloracetic acid.....	25.921 [1025]	25.677 [1017]	0.244 [8]
Trichloracetic acid ...	26.022 [1038]	26.732 [1038]	0.290 [0]
Propionic acid	25.652 [1017]	25.501 [1005]	0.151 [12]
Butyric acid	25.617 [1009]	25.477 [999]	0.140 [10]
Isobutyric acid	25.572 [1012]	25.444 [1006]	0.137 [6]
Glycollic acid	26.112 [—]	26.013 [—]	0.099 [—]
Lactic acid	26.006 [1014]	25.872 [1005]	0.134 [9]

TABLE X.
Differences referred to Bases.

	Potash.	Soda.	Ammonia.
Nitric acid	13.735 [643]	13.696 [640]	12.829 [619]
Hydrochloric acid ...	13.220 [628]	13.064 [631]	12.769 [610]
Hydrobromic acid ...	13.325 [662]	13.162 [659]	12.705 [641]
Hydriodic acid.....	13.498 [684]	13.361 [675]	12.828 [658]
Formic acid	6.060 [230]	6.019 [230]	5.677 [222]
Acetic acid	3.221 [109]	3.613 [106]	5.009 [112]
Monochloracetic acid..	4.554 [171]	4.454 [164]	4.183 [155]
Dichloracetic acid ...	6.645 [198]	6.528 [196]	6.295 [185]
Trichloracetic acid ...	13.056 [520]	12.893 [526]	10.605 [494]
Propionic acid	1.520 [64]	1.505 [58]	1.448 [59]
Butyric acid	0.683 [25]	0.670 [21]	0.637 [25]
Isobutyric acid.....	0.000 [00]	0.000 [00]	0.000 [00]
Glycollic acid	3.315 [—]	3.343 [—]	2.774 [—]
Lactic acid	1.966 [99]	1.959 [96]	1.531 [81]

The constancy in the differences of the numbers found for

acids and for bases is accounted for by Ostwald by the hypothesis that *the change of physical properties brought about by each substance entering into chemical combination is of constant value, and is independent of any alteration caused by the entrance into the compound of other substances.*

The volume-changes which accompany the action of monobasic acids upon the normal salts of the same acids are shown to be very small. The volume-changes accompanying the action of one monobasic acid upon the normal salt of another acid are determined by Ostwald; and by dividing the number so obtained by the difference between the volume-changes noticed on neutralizing the base by each acid separately (a small correction being made for the action of the acid set free in the reaction upon its own normal salt), numbers are obtained representing the percentage amount of base taken up by the added acid. These numbers are contained in the following Table, in which the first column contains the name of the normal salt, and the second that of the free acid added. One equivalent of free acid is always added to one equivalent of normal salt.

TABLE XI.

Division of Base between two Monobasic Acids.

		Potash.	Soda.	Ammonia.	Mean.
Dichloracetate.	Nitric acid	77	77	75	76
Do.	Hydrochloric acid ...	74	75	73	74
Do.	Trichloroacetic acid ...	{ 70 73	{ 71 71	{ 70 72	71
Do.	Lactic acid	8	9	11	9
Monochloracetate.	Trichloroacetic acid	92	92	92	92
Formate.	Trichloroacetic acid...	97	96	97	97
Do.	Lactic acid	43	46	48	46
Do.	Acetic acid ..	25	23	23	24
Do.	Butyric acid	21	21	19	20
Do.	Isobutyric acid	19	19	18	19
Butyrate.	Acetic acid	54	52	53	53
Isobutyrate.	Acetic acid	56	51	53	53
Propionate.	Formic acid.....	78	80	79	79
Glycollate.	Formic acid.....	43	44	45	44

A similar table is given by Ostwald containing his results obtained by the chemico-optical method; the general results agree very well with those of Table XI.

Taking the affinity of nitric acid as 100, and that of hydro-

chloric acid as 98*, the affinity of dichloracetic acid is $\frac{24}{76} \times 100 = 32$, or, starting from hydrochloric acid, $\frac{26}{76} \times 98 = 34$.

In this manner the following Table is framed:—

TABLE XII.
Relative Affinities.

Nitric acid.....	100	Formic acid	3.9
Hydrochloric acid.....	98	Lactic acid	3.3
Trichloracetic acid	80	Acetic acid	1.23
Dichloracetic acid.....	33	Propionic acid	1.04
Monochloracetic acid ...	7.0	Butyric acid.....	0.98
Glycollic acid.....	5.0	Isobutyric acid.....	0.92

The *order* of the acids as arranged in this Table is regarded by Ostwald as correct; but he believes that further researches will necessitate considerable changes in the numbers for the individual acids.

The entrance of chlorine largely increases the affinity of the acid: this is shown by the numbers 1.23, 7.0, 33, and 80 for acetic, monochloracetic, dichloracetic, and trichloracetic acids respectively. Similarly the entrance of oxygen into the molecule of the acid increases the affinity†; while the addition of CH_2 decreases the affinity, as shown in the series of acids from formic to butyric, and also in glycollic and lactic acids.

The importance of the results obtained by Guldberg and Waage, and by Ostwald, must be apparent to every chemist.

In both series of researches we are taught of a coefficient of affinity which is a fixed quantity for each chemical molecule; but at the same time we are led to recognize the paramount importance of the physical conditions under which this affinity is exercised.

We are further presented with a tolerably satisfactory explanation of the leading facts of chemical action, without the necessity of appealing to any special and mystical “force of attraction” to account for these facts.

The modern advances in the theory of chemical action lead us back more to the writings of Berthollet than to those of Bergmann. Bergmann’s idea that the affinity of different substances may be represented by the amounts of each which combine together, was evidently untenable in the fuller light

* This result is arrived at by Ostwald’s own researches and by those of Thomsen, Pogg. *Ann.* cxxxviii. 65.

† This is further shown by the numbers obtained for the relative affinities of succinic, malic, and tartaric acids, which are 1.45, 2.82, and 5.2 respectively.

of the atomic theory. A modified form of Berthollet's assertion that the affinity of an acid is greatest for that base with which it combines in smallest quantity, seems to be suggested by Thomsen's results (confirmed by Ostwald's volumetric method) that large affinity-value is accompanied by low heat of neutralization.

Ostwald furnishes chemistry with a new method for solving some of her most difficult problems; and Guldberg and Waage lead the way in the application of mathematical reasoning to the facts of chemical science.

XXIII. *The Distribution of Heat in the Visible Spectrum.*

By Sir JOHN CONROY, *Bart., M.A.**

IN a paper "On the Distribution of Heat in the Spectrum," originally published in the *Philosophical Magazine* for August 1872, and since reprinted in a volume of 'Scientific Memoirs,' Dr. J. W. Draper states the theoretical reasons for supposing that all the rays in the spectrum have the same heating effect, in the following words:—"A given series of waves of red light impinging upon an extinguishing surface will produce a definite amount of heat; and a similar series of violet waves should produce the same amount; for though an undulation of the latter may have only half the length of one of the former, and therefore only half its *vis viva*, yet in consequence of the equal velocity of waves of every colour, the impacts or impulses of the violet series will be twice as frequent as those of the red. The same principle applies to any intermediate colour; and hence it follows that every colour ought to have the same heating effect."

Dr. Draper gives an account of some experiments he has made on the distribution of heat in the visible spectrum of sunlight. He finds that if the visible spectrum between A and H_2 be divided into two equal portions, and all the light of wave-lengths between 7604 and 5768 be collected together, and also all that of wave-lengths between 5768 and 3933, the heat-intensity of these two series of undulations as determined by the thermopile are equal.

The distribution of heat in the spectrum of sunlight had been previously experimentally investigated by Sir W. Herschel (*Phil. Trans.* 1800, p. 255), J. Müller (*Pogg. Ann.* cv.

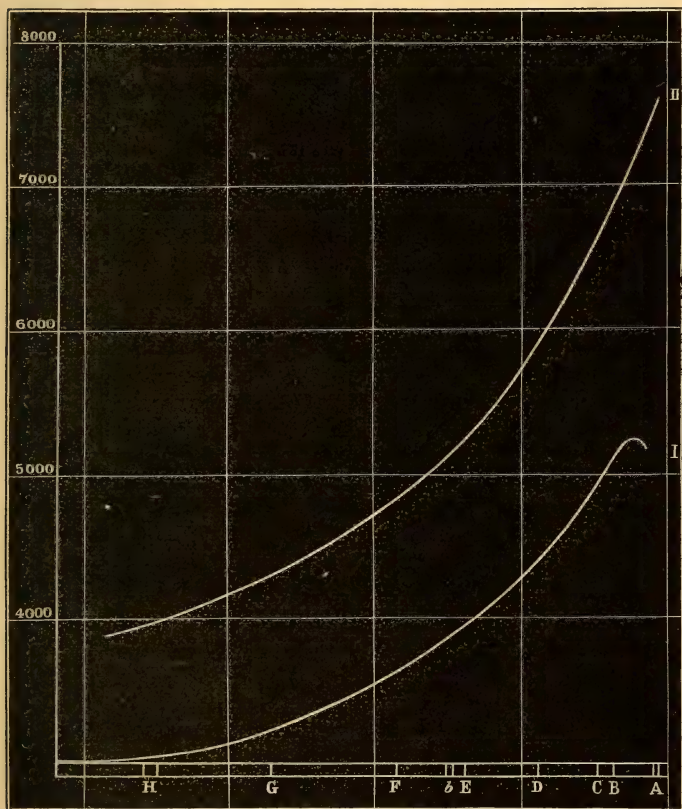
* Communicated by the Physical Society, having been read at the Meeting held on June 28.

p. 337), Franz (Pogg. *Ann.* cxv. p. 266), Knoblauch (Pogg. *Ann.* cxx. p. 177, and cxxxvi. p. 66), Fizeau and Foucault (*Comptes Rendus*, xxv. p. 447, and reprinted in the *Annales de Chimie*, 5th series, xv. p. 363), Desains (*Comptes Rendus*, lxvii. p. 297, and lxx. p. 985), Lamansky (Pogg. *Ann.* cxlvi. p. 200). Similar measurements were made with the limelight by Desains (*loc. cit.*) and Lamansky (*loc. cit.*), and of the electric light by Professor Tyndall (*Phil. Trans.* 1866, p. 1). They all found but slight indications of heat in the violet and blue regions of the spectrum, the amount increasing in the green, yellow, and red, and attaining a maximum at a point beyond the end of the visible spectrum.

The experiments were all made by placing a thermometer (one of the ordinary construction being used by Sir W. Herschel and MM. Fizeau and Foucault, and a thermopile and galvanometer by the other observers) in various parts of the dispersion-spectrum formed by prisms of either glass, rock-salt, or sylvine. As Dr. Draper points out in the paper already referred to, this method appears to be an essentially defective one, as, owing to the unequal dispersion by the prism of rays of different refrangibility, a greater number of undulations of different wave-lengths must have been incident upon the surface of the thermometer when it was placed in the red and yellow portions of the spectrum than when placed in the green, blue, or violet portions; and the amount of heat indicated by the instrument being in proportion to the amount of radiant energy incident upon its surface, the unequal dispersion of the prism would be sufficient to account for some difference in the heating effects produced by different portions of the spectrum.

A graphical method appearing to afford the readiest means of determining the probable effect produced by the unequal dispersion of the prism, a tracing was made, on paper divided into squares of $\frac{1}{10}$ inch, of the curve representing the intensity of the heat in different portions of the visible spectrum, as determined by MM. Fizeau and Foucault (*Ann. de Chim.* 5 sér. xv. p. 377)—the position of the fixed lines in the spectrum, as given by them, being marked on one edge of the paper, which was taken as the x axis, and a scale of wave-lengths in “tenths-metres” laid down at right angles to this, and the curve for the dispersion of the prism constructed in the ordinary manner. At nineteen equidistant points in the spectrum the ordinates of the dispersion-curve were measured in wave-lengths; the difference between any two of them gave, approximately, the dispersion of the prism for that portion of the spectrum. A

difference of wave-length of 100 was taken as the unit, and the difference between the values of the ordinates divided by 100 considered as a measure of the dispersion. The ordinates



of the heat-curve of MM. Fizeau and Foucault at eighteen points in the spectrum, midway between those at which the ordinates of the dispersion-curve had been taken, were measured in tenths of an inch, these numbers divided by those representing the dispersive power of the prism, and the quotients taken as giving the true relative intensity of the heat at the different points of the spectrum.

Ordinates of the dispersion-curve.	Difference divided by 100 (α).	Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$.
7260			
6790	4.70	22.4	4.7—B.
6320	4.70	19.0	4.0—C.
6000	3.20	15.9	5.0
5750	2.50	13.7	5.5—D.
5500	2.50	11.8	4.7
5260	2.40	10.3	4.3—E.
5070	1.90	8.8	4.6— <i>b</i> .
4900	1.70	7.5	4.4—F.
4750	1.50	6.2	4.1
4610	1.40	5.0	3.5
4490	1.20	4.0	3.3
4380	1.10	3.3	3.0
4270	1.10	2.5	2.2—G.
4170	1.00	1.8	1.8
4070	1.00	1.3	1.3
4000	.70	1.0	1.4
3930	.70	.7	1.0—H.
3880	.50	.5	1.0

The Table shows that the numbers thus obtained for the intensity of the heat in different portions of the spectrum lie close together for the region between B and F, the maximum being near D, and that from F to H the intensity diminishes.

It would further appear that the curve for the distribution of heat of MM. Fizeau and Foucault is in reality a dispersion-curve, drawn to some scale of wave-lengths, for the particular prism used by them; and the diagram shows how very similar the curves for the intensity of the heat, and for the dispersion of the prism are to one another.

The heat-curve reaches the x axis at a short distance on the more refrangible side of H_2 , the wave-length of that line being 3932. The axis was taken as 3900 on the scale of wave-lengths to which the curve is drawn, and therefore the ordinate of the curve at B, measured in tenths of an inch, as proportional to the difference between this number and the wave-length of B, and the height of the ordinates at the other lines calculated on this assumption.

	Measured.	Calculated.	Difference.
B	2.10		
C	1.87	1.88	+ .01
D	1.34	1.41	.07
E	.95	.97	.02
<i>b</i>	.88	.90	.02
F	.63	.68	.05
G	.22	.29	.07

Mean difference ... +.04

Considering the nature of the data, and especially the small scale of MM. Fizeau and Foucault's diagram (the portion representing the visible spectrum being only about 4 inches long), the measured and calculated numbers agree fairly well together.

The same process was applied to the curves given by Lamansky (Pogg. *Ann.* cxlvi. p. 200) for the distribution of heat in the solar spectrum, with flint-glass and rock-salt prisms.

Flint-glass prism.			Rock-salt prism.		
Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$		Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$	
22.8	8.4	—D.	18.2	6.0	—D.
21.0	9.5		16.4	6.0	
19.0	11.8	—E.	14.3	7.9	—E.
17.5	19.4		13.0	9.2	—E.
16.0	20.9		11.5	8.2	
14.5	20.7	—b.	10.3	7.3	
12.7	15.8		9.3	7.1	—F.
11.2	14.0		8.2	5.8	
10.3	14.7	—F.	7.0	5.0	
9.8	14.0		6.2	4.4	—G.
9.3	13.2				
8.5	12.1				
7.6	10.8				
6.8	8.5				
6.0	7.5				
5.0	7.1	—G.			

Assuming the curves to be the dispersion-curves for the prisms, the ordinates were measured and calculated as in the former case.

Flint-glass prism.			Rock-salt prism.		
	Measured.	Calculated.		Measured.	Calculated.
D	23.6		D	18.3	
E	17.8	16.0	E	13.7	13.4
b	15.4	14.6	F	9.8	10.3
F	10.2	10.8	G	6.0	
G	4.0				

In none of the other measurements that have been made of the heat-spectra, as far as I am aware, are the positions of the solar lines stated; and therefore part of the data for eliminating the action of the unequal dispersion of the prisms is wanting.

An attempt was made to deal with Knoblauch's measure-

ments of the solar spectrum, and with Tyndall's of that of the electric light, by the same method.

In both cases the experiments were made with rock-salt prisms; and these were assumed to have the same dispersive power as the one used by Lamansky, and the curve plotted accordingly. Two sets of measurements are given by Knoblauch; and the mean of these was taken for the following calculations.

Knoblauch's measurements.

Ordinates of heat-curve (β).	$\frac{\beta}{\alpha}$.
16.1	2.3
12.0	3.6—D.
8.7	3.9
7.5	4.1
7.0	4.6—E.
6.8	4.8
6.6	4.7
6.3	4.5—F.
6.0	4.6
5.7	4.0

Tyndall's measurements.

Ordinates of heat-curve (β).	$\frac{\beta}{\alpha}$.
28.3	3.0
21.0	3.0
15.7	4.7—D.
12.0	5.4
8.2	5.7
6.5	4.3—E.
5.0	3.5
3.5	2.5
2.0	1.7—F.
1.5	1.1
.9	.6

The nature of the available data is such that the only definite conclusions which it is possible to draw from these calculations are, that the distribution of heat in the normal spectrum differs greatly from that in the dispersion-spectrum, and that in the dispersion-spectrum the great calorific intensity of the red rays, and therefore in all probability of the invisible rays beyond them, is due to the action of the prism in concentrating these rays upon the face of the thermopile. The intensity of the heat in the different portions of the normal spectrum, except in the case of Lamansky's measurements with the flint-glass prism, apparently varies but little through a considerable space; and this affords some support to Dr. Draper's hypothesis, that every colour ought to have the same heating effect.

After I had finished these calculations, I found that G. Lundquist had investigated (*Pogg. Ann.* clv. p. 146), from Lamansky's measurements, the distribution of heat in the normal spectrum, and had shown that it differed greatly from the distribution in the dispersion-spectrum—the maximum intensity in the case of the flint-glass prism being near D, and in the rock-salt prism near E. He also found from Tyndall's

measurements of the heat of the electric-light spectrum, that in the normal spectrum the maximum was near A.

Lundquist arrived at these results by a mathematical process, based on the same general principles as the graphical one I have employed.

XXIV. *On Structures in an Earthquake Country.* By JOHN PERRY and W. E. AYRTON, *Professors in the Imperial College of Engineering, Japan**.

IN a country like Japan, where several sharp earthquakes occur yearly, where there are between three and four hundred destructive earthquakes on record, and where over a hundred thousand people are said to have been killed in one almost continued earthquake lasting for a month, and which occurred so recently as 1855, the question of the stability of structure is all-important.

When working at our paper "On a neglected Principle that may be employed in Earthquake Measurements," read before the Asiatic Society of Japan on the 23rd of May, 1877, and which appeared in the Number of the Philosophical Magazine for July 1879, we were led to consider how the effect produced by an earthquake on a structure is influenced by the time of vibration of the structure.

It follows from that principle that if a number of quickly vibrating bodies form part of the same structure, they all vibrate in much the same way; that is, the periods of their swings are all approximately equal to one another and equal to the periods of the earthquake; and although they differ in the amount of their motions, these amounts and their differences are all exceedingly small; whereas if one or more of the parts of the structure are only capable of vibrating slowly, the periods of vibration of the different parts vary very much, the amounts of the motions are all comparatively great, and their differences are all relatively considerable. If, however, there is a sufficiently great viscous resistance to motion of such slowly vibrating parts, these parts will be found during an earthquake to behave much as if their natural periods of vibration were quick. Supposing the foundation of a structure to vibrate with the earth which encloses it, we see that a slowly vibrating structure which is fastened to these foundations is during an earthquake subjected to stresses which may be excessively great and of a very complicated kind, whereas a quickly vibrating structure is subjected to stresses which may be said to be de-

* Communicated by the Authors.

terminate, and which are comparatively small. It is not here necessary to consider whether, as all the motions of a quickly vibrating body must be small, such a structure will be more comfortable to live in, because it is doubtful whether the annoyance produced by rapidity of shock would not more than counterbalance the annoyance of great but smooth motions. It is only safety we are here considering; and in this respect there can be no doubt of the superiority of rigid structures, or of structures having a sufficiently great viscous resistance to motion. Some calculations which we have made of the times of vibration of ordinary structures, such as well-built houses of stone and brick, chimneys, lighthouses, &c., will be found at the end of this paper; and from these we see that the periods are all much less than what we judge from our experience is the ordinary period of vibration of earthquakes in Japan. Even two-storied houses built of wood, if framed in the best way, have quick times of vibration; such structures are therefore, it seems to us, well capable of resisting the ordinary Japanese earthquake-shock. As, however, we have not yet experienced the effects of a destructive earthquake, and as we presume that one of the most important ways in which it may differ from ordinary earthquakes is in the suddenness of motion, or change of motion, it cannot be said that any ordinary structure has a quicker period of vibration than a destructive earthquake; consequently, if it be granted that stability depends on the structure having a quicker period of vibration than that of the earthquake, the stability of a building will be only relative. We can of course be sure that by making the walls of a building thicker and its height less, we add to its safety; but however far we may go in this direction, we cannot be certain but that after all the earthquake-period may be less than that of our building.

We must therefore content ourselves with saying that a slowly vibrating structure will probably get broken in its connexions with the foundations if these be rigidly fixed to the ground: consequently (and we here oppose the practice of many architects and engineers) putting a heavy top to a lighthouse, the chimney of a factory, or other high building, must certainly take from its stability.

And although we see from the calculations at the end of the paper to which we have referred, that the times of vibrations of ordinary brick and stone houses are very short, still, in view of the possible great suddenness of a destructive earthquake, we should advise that all buildings be kept as low and made as rigid as possible.

The argument used by engineers to support the practice

above referred to, of placing a heavy top on a chimney, assumes that the shock is an impact, and consequently that a definite quantity of momentum is given to the structure; but it must be quite evident that it is the relative velocity of the base of the structure with regard to the other parts which is the fixed quantity, and therefore that the more massive the structure, the more momentum enters it through the base.

There is no easy way of judging what are the forces which cause an ordinary Japanese house to return to the perpendicular position after it has received a push or blow; and so we cannot calculate its natural time of vibration; but it is well known that it vibrates very slowly, an ordinary Japanese two-storied house with the usual heavy roof taking perhaps four seconds to make a complete vibration. The restoring forces are due merely to stiffness of the joints, there being no rigid connexion with the ground, since the vertical posts of the house are all supported on detached stones, and there are also no diagonal stays in the building. Such a structure is therefore capable of being displaced very far from its position of equilibrium without fracture occurring; and as its time of vibration is very long, it has a very great amplitude of swing during most ordinary earthquakes. That this amplitude is not even greater is most probably due to the fact that there is a sort of viscous resistance to motion at all its joints. Such a viscous resistance must greatly diminish the motion, and will be especially useful in an earthquake consisting of regular vibrations; but the most severe test of such a structure consists in an earthquake-shock which begins with a sharpe impulse, or which has a very irregular motion. The slowly vibrating structure would register the shock in a longer period of time than that in which the blow was delivered; but it would probably have an exceeding great first swing from its position of rest.

We think that the important elements of safety in ordinary Japanese structures is this viscous resistance which they oppose to motion, and which is mainly due to the great multiplicity of joints (all of which are compelled to move) and to the absence of diagonal pieces; for we deduced from the principle in our original paper, that if the restoring forces are weak there ought to be a great viscous resistance to motion if we wish the strains of the structure to be small. But it must be remembered that this safety is only gained by a very great expenditure of timber; so that, although such slowly vibrating structures as many of the temples in this country may be regarded as exceedingly safe during earthquakes, it must not be concluded that all heavily roofed houses are secure.

The amount of momentum which has to be transmitted through the foundations of a building to the superstructure depends on the nature of the earthquake (that is, its suddenness and the amount of earth-motion), as well as on the mass of the building, while the velocity of the foundations, if these are rigidly connected with the earth, is independent of the mass of the building—an important fact to which we have already drawn attention. The earthquake energy gets destroyed by the interior portions of the earth, as well as the mountains and buildings at its surface not having exceedingly small periodic times of vibration, in consequence of which interference takes place at every surface of contact of the different portions. Of course, however, any one particular building will destroy only a very small portion of the whole energy of the earthquake-vibration; so that its mass cannot in any perceptible way affect the motions of its foundations.

In the same way as we have shown that the more quickly a house is capable of vibrating the less is its motion relative to the foundation, we might arrive at the result that the smaller the natural period of vibration of the several portions of a body subjected to shocks, the less internal friction must there be; and this conclusion is consistent with the well-known fact that there is more internal friction in non-homogeneous bodies, or rather, we should say, in bodies which, being non-homogeneous, have some of their materials only capable of very slow natural vibrations compared with the remainder.

We have no doubt that with any given material whatever there is a best method of constructing buildings in an earthquake country. Thus, with small stones set in bad mortar, or in no mortar, as in the buildings destroyed by the Neapolitan earthquake of 1857, the momentum which must pass through any level joint depends (1) on the short time t during which the foundations are acquiring a great velocity v , (2) on the mass of the building M above the joint, and (3) on the natural time of vibration of the portion of the structure between the given joint and the foundations. If this time of vibration is very short, then the momentum Mv must be transmitted by the joint in the short time t ; that is, the joint must transmit the great force $\frac{Mv}{t}$; whereas if the time of vibration of the building below the joint is considerable, the time of transmission of momentum is increased in a calculable way, say to the time nt , and hence the force transmitted by the joint becomes reduced to $\frac{Mv}{nt}$. It is for this reason that, if we

wish to drive in a nail without hurting the head with the hammer, a block of wood is used as a cushion, the wood being of service because, having an appreciable time of vibration, it causes the duration of the impact to be lengthened, and so diminishes the force acting at any moment. Similarly, the barometers in men-of-war are not now suspended direct to the ship's sides, but to the end of a flexible lath, in order to prevent the shock accompanying the firing of the guns breaking the barometer-tube. In the same way, the lower parts of a structure having appreciable times of vibration cause the earthquake-shock to be altered in character, to be lengthened in time, and therefore diminished in intensity before it reaches the upper parts. Hence it is obvious that if small stones or bricks set in bad common mortar are our building materials, it would be better to choose for the site a quaking bog which was capable of supporting the weight of the building, rather than to build the house direct from a rocky foundation; or if the ground is firm, there ought to be placed underneath the house a foundation of yielding timber; or some other method should be sought for by means of which the time of transmission of momentum through the joints may be increased.

Thus, there is a best time of vibration of the part of a structure below a joint, which depends on the strength of the joint; and if the basement has a time of vibration different from this, then we should advise that the building be kept low. For example, it is desirable that houses with ordinary wall-thicknesses built of bricks set in common mortar should not be more than one or, at the very most, two stories high if there is a piled or concrete foundation; but if good cement be employed instead of bad mortar in fastening the bricks together, then a height of two or three stories may be employed probably with comparative safety.

Again, the horizontal vibration of the ground is given up to a stone or brick building mainly by shearing-stress communicated from course to course, a kind of stress which mortar is very unsuitable to transmit. Hence a stone or brick building subjected to horizontal shocks ought certainly to be built with cement and not with ordinary mortar. In fact, in every part it ought to be capable of resisting pulling as well as crushing stresses.

Every joint is a weak place; and it is evident that if by increasing the size of the building we diminish the area of joints, we shall be increasing the stability. Now in large masonry structures larger stones are, as a rule, employed, and the joints are made of less area. In this respect, then, may we say that

large masonry structures built with common mortar are usually more stable than smaller ones.

It is quite evident that as a concrete can be obtained which will resist as great a tensile stress as ordinary brick itself, we shall derive great benefit from making all horizontal sections, of a structure which is composed of bricks set in good cement, as great as possible; that is, we shall find that the most suitable structure, if of brick or stone, for an earthquake country must be composed of large stones set in good cement with walls as thick as possible near the base, the thickness of wall at every place being roughly proportional to the mass of the building above that place.

As, however, the resistance to tension of timber is very much superior to that of cement or bricks, and as the mass of a timber building is small, a timber building with sufficiently strong joints must be very much superior to any structure of brick or masonry. And for the same reason a building of wrought iron might be made stronger still, and one of steel strongest of all.

Ordinary timber houses ought not to be too rigidly fastened to the earth: if the joints of the structure are made, however, very strong, and especially if wrought iron is used as well as wood, and if there is diagonal bracing, then the connexions with the ground may be made more rigid. The stiffness of structures varies so much that we cannot give more definite rules than those contained in this short article; but it is obvious that our principle of relative vibrations may be easily applied to find the best arrangement in a structure for any given material and with any given foundation.

Calculations of Times of Vibration of different Buildings.

Since a square or circular building has usually the same period of vibration in all directions perpendicular to its height, it is not necessary to specify in which direction it is vibrating. Let us consider a prismatic structure of height h well built into the ground and of uniform horizontal section A . Let K be the radius of gyration of the section about an axis through the centre of the building; then, taking into account bending and shearing stresses, a horizontal force P applied at the centre of gravity of the prism produces a deflection of the centre of gravity equal to

$$\frac{P}{EAK^2} \cdot \frac{h^3}{24} + \frac{P}{A} \cdot \frac{h}{2N},$$

where E is the modulus of elasticity of the material, and N the modulus of rigidity, which latter is about one third of the former

for building material. Consequently the deflection of the centre of gravity is equal to

$$\frac{Ph}{EA} \left\{ \frac{h^3}{24K^2} + \frac{3}{2} \right\};$$

so that if, for simplicity, we suppose the prism to vibrate as if its mass were gathered at its centre of gravity, if T is the period of a complete natural vibration, and if w is the weight of the material per unit volume,

$$T = 2\pi \sqrt{\frac{\frac{Ph}{EA} \left\{ \frac{h^2}{24K^2} + \frac{3}{2} \right\}}{\frac{gP}{hAw}}},$$

or

$$T = 2\pi \sqrt{\frac{h^2 w}{gE} \left\{ \frac{h^2}{24K^2} + \frac{3}{2} \right\}}.$$

For a solid rectangular structure with a square horizontal section of breadth b ,

$$K^2 = \frac{b^2}{12},$$

or

$$T = 2\pi \sqrt{\frac{h^2 w}{gE} \left\{ \frac{h^2}{2b^2} + \frac{3}{2} \right\}}.$$

For a hollow square section, the sides of the outer and inner square being respectively a and c ,

$$\begin{aligned} K^2 &= \frac{a^4 - c^4}{12(a^2 - c^2)} \\ &= \frac{a^2 + c^2}{12}; \end{aligned}$$

$$\therefore T = 2\pi \sqrt{\frac{h^2 w}{2gE} \left\{ \frac{h^2}{a^2 + c^2} + 3 \right\}}.$$

Now for brick

$$w = 112 \text{ lbs.}$$

and $E = 144,000,000 \text{ lbs. per square foot}$ } about;

$$\therefore T = 0.001534h \sqrt{\frac{1}{2} \left(\frac{h^2}{a^2 + c^2} + 3 \right)}.$$

Square house with no roof.

Let $h=30$ feet, $a=30$ feet, $c=26$ feet; then

$$T = 0.001534h \sqrt{\frac{1}{2} \left(\frac{900}{900 + 676} + 3 \right)}$$

$$= 0.06120 \text{ second about.}$$

This result may be taken as approximately correct even for roofed houses, because in a small house the roof gives stiffness as well as adding inertia.

When the height of the building is not more than twice or three times the outside horizontal dimensions, the shearing strains are important, and must be included in the calculations, as we have done. But when the height becomes eight or more times the horizontal dimensions, as in the case of a chimney, then we may neglect shear stress and consider bending only.

Tall Chimney.

Let $h=150$ feet, $a=10$ feet, and $c=4$ feet—that is, let us consider a thick square chimney; then

$$T = 0.001534 + 150 \sqrt{\frac{1}{2} \left(\frac{22500}{116} + 3 \right)},$$

$$= 2.301 \text{ seconds, about.}$$

If the chimney has the same internal dimensions, but if the brickwork is thinner than we have taken it, then a is less and T becomes still greater. Even, therefore, without the heavy top that some engineers have recommended to be added, this structure vibrates too slowly to be sufficiently safe in a country visited by frequent earthquakes, like Japan; and we think it quite likely that the first really severe shock which may be experienced by the various chimneys recently erected in this country will destroy them. It may here be noticed that even in England it is not thought to be safe to connect a tall factory-chimney with the main walls of a building; so that, remembering the very great difference in the time of vibration of the chimney and the walls, such a connexion must be regarded as exceedingly unsafe in Japan.

Mountain.

Let us consider a conical mountain, then it is clear that in its vibrations shearing-stresses need alone be considered. A horizontal force P acting at the centre of gravity produces the deflection

$$\int_{\frac{2}{3}h}^h \frac{P}{\pi r^2} \frac{dx}{N},$$

where r is the radius of the mountain at any distance x from the vertex, and N the modulus of rigidity.

Now if a is the radius of the base, then at any distance x from the vertex the radius is

$$\frac{x}{h} a.$$

Assuming, as before, that N is equal to one third of E , we have for the deflection produced by the horizontal force acting at the centre of gravity of the mountain

$$\int_{\frac{2}{3}h}^h \frac{3Pdx}{\pi \frac{x^2}{h^2} a^2 E},$$

or

$$\frac{3Ph}{2\pi a^2 E}.$$

Consequently

$$T = 2\pi \sqrt{\frac{h^2 w}{2gE}}$$

$$= 0.00107 \times h \text{ approximately.}$$

Hence a cone in which the diameter of the base is not much less than its height makes a complete vibration in one second if its height is 1000 feet; and the times of vibrations of such cones and pyramids are proportional to their heights.

A large cone, however, would not receive the earthquake-shock as a house does, because the house receives the vibration at every portion of its base almost simultaneously; so that it is difficult from the equation concerning the vibrations of buildings to predicate the production of cracks at the base of mountains. In fact, the question must be treated in a different way, the propagation of vertical and horizontal strains being considered. It is evident, however, that unless the earthquake is of a suddenness such as we cannot comprehend, no fracture will be produced at the base of a hill or pyramid of, say, 100 feet in height.

XXV. *On the Passage of the Galvanic Current through Iron.*
By FELIX AUERBACH, Ph.D., of Breslau.

[Continued from p. 152.]

§ 8. **W**E have yet to reply to the question *whether the theory above sketched conditions an influence upon the fundamental laws of galvanism, as well as upon the galvanic constants of iron*, and, in the case of an affirmative answer, how this influence asserts itself.

That such an influence must be present the following consideration shows. In the fundamental laws, among other ideas occurs that of resistance; this must therefore be fixed once for all, in order that the validity of those laws may have a meaning. For iron, according to the investigation which has been carried out, this is a doubtful problem. Over against the reflection that, in order to make comparisons with other metals possible, the resistance of unmagnetic iron must be taken into consideration, stands another, that this quantity is inaccessible to experience. Since, then, the laws of Ohm, Joule, and Lenz, &c., hold good for it alone, experience, which of necessity makes use of the empiric notion of resistance, must give deviations from the laws.

According to Ohm's law the resistance is independent of the electromotive force*. This of course is true also of the *ideal* resistance of an iron wire; *its real resistance must, on the contrary, change when the electromotive force, and with it the current-intensity, is changed.* For with the latter the circular magnetization, and with this the resistance, increases up to the limit of saturation.

In order to test this requirement experimentally, I made use of an arrangement which permitted the electromotive force to be varied instantaneously. This was a plug commutator constructed upon a plan of Professor Meyer's. Upon a hardgum plate the brass pieces represented in the shaded part of fig. 5 (Pl. I.) are fixed. By inserting metal plugs in the conic apertures they can be connected by means of the binding-screws A, B, &c. with one another and with the other parts of the circuit. If, for example, a galvanic element (the positive pole is always first mentioned) be connected with E and B, a second with A and H, a third with G and F, and of the plugs only those at *a, d, e, and h* be fixed, while the binding-screws C and D serve merely for carrying off the current, the three elements are inserted one after another. If now the plugs at *a* and *h* be taken out, and plugs put in at *b, c, f, and g*, the three elements are inserted side by side†.

For my purpose I inserted the commutator at A and C in

* According to the last Report made to the British Association, for copper this law agrees perfectly, so far as the accuracy of observation extends.—*Beibl.* ii. p. 267 (1878).

† Let me be permitted to take this opportunity to recommend for lecture-purposes the above-described and another commutator, likewise proposed by M. Meyer, of which the drawing fig. 6 is an illustration. A combination of *one* specimen of the *first* and *two* of the *second* sort makes it possible for the lecturer to pass over at pleasure from any galvanic experiment to any other by merely transposing the plugs. M. Pinzger, of this place, supplies them excellently executed.

one diagonal of the Wheatstone square ; between B and D a Daniell's element (in the order of succession copper—zinc) was inserted, between F and H two, and between G and E three such elements in the same direction and one behind another. The plug at *b* was never put in, those at *a* and *h* always; if beside these there were now fixed in place only

g and *f* *c, e, f* *c, g, d* *g* and *d* *c, e, d* *e* and *d*

then there were

1 2 3 4 5 6

elements inserted.

The double and triple elements can thus be simply excluded; the single one can, it is true, be only weakened by the secondary closing at *c*; I found, however, that in reality it is thereby likewise excluded.

The resistance in the elements hardly came into consideration in comparison with that of the rest of the circuit; the intensity of the current therefore varied nearly as the electromotive force. I at first intended to make these experiments with the wires already used for the others; but they exhibited a behaviour so *abnormal* that I was obliged to replace them by fresh ones that had not been submitted to galvanic and magnetic treatment. I will cite only a few instances of these abnormalities. Through a thin annealed iron wire (the magnetization-experiments made with which are not included among those above selected) the current of 2D was sent, without intermission, for one minute each time, first in one and then in the other direction. The following resistances *a* and *b* were found:

<i>a</i>	14.57	14.42	14.29	14.23	14.14	14.11	14.06	14.06	14.03
	14.32	14.03	14.00	13.98	13.96	13.95	13.94	13.94	
<i>b</i>	13.74	13.37	14.15	13.71	13.38	13.69	13.43	13.81	13.39
	13.82	13.44	13.89	13.44	13.81	13.51	13.79	13.39	

While, therefore, *a* apart from a gradual diminution, amounting to about 4 per cent., connected with the gradual lowering of the surrounding temperature, shows constancy, the values of *b* form a tolerably regular zigzag line, the maxima of which deviate on the average 3 per cent. of the entire ordinates. Further, the next day the mean resistance (the wire being differently fixed), on the employment of

$$\left. \begin{array}{l} 1D, w_1 = 14.92 \\ 2D, w_2 = 15.63 \\ 3D, w_3 = 16.33 \end{array} \right\} \begin{array}{l} w_2 - w_1 = 0.71 \\ w_3 - w_2 = 0.70 \end{array}$$

was found to be

$$\frac{w_{n+1} - w_n}{w_n} = \delta = 0.048.$$

So colossal an alteration of resistance (almost 5 per cent.) could not have escaped the observers, and is moreover in itself improbable; it even overpasses the limits of the alterations of resistance by magnetizings as above given (§ 6).

A third series of experiments, finally, showed that when the current passed through continuously the resistance diminished enormously; there was, namely:—

At first.	After 1 min.	Again, after 3,	5,	5,	5 minutes.
$w = 16.54$	16.23	16.01	15.92	15.83	15.82 (constant).

The current was then opened for a short time, and again closed; a reiterated diminution of w was the result; at $w = 15.70$ constancy appeared to have come in again; and so it went on. All these phenomena may at once be characterized as consequences of the disturbed molecular relations of the wire.

Even with new wires there is *one* difficulty not unimportant. By every alteration of the electromotive force the *thermal equilibrium* of the wire is disturbed, as the radiation for some time does not keep pace with the increased heating. In one branch of the bridge, however, which consists exclusively of German silver, the heating has very little, but in the other, in which is the iron wire, an important influence. I have endeavoured to approximately determine this influence from the numerical data supplied by Weber, Favre, and Bosscha. In Bosscha's units the electromotive force of a Daniell is in round numbers 10^{11} , therefore in Volta's units of current and Siemens's resistance-units 10. Now, in operating I constantly added only one element (never more) at a time. We shall therefore obtain an upper limit for the heating if we calculate the heating by 2D; that effected by 1D is not sufficient, because the heating increases as the square of the electromotive force at constant resistance. We have thus the electromotive force $E = 20$; and the mean of the total resistance of the two sides of the tetragon through which the current flowed was exactly the same—namely, 10 in the comparison branch (w_2 , *conf.* § 3), and 10 on the average in the iron wire. The current-intensity is therefore $= 1$. At the same time, according to Favre, 1.6 unit of heat is generated, therefore 0.8 in the iron wire. The weight of the latter amounted to at least $10 \times 0.1 \times 10000 = 10000$ milligrams, or 10 grams. These 10 grams of iron are about as much heated as 1 gram of water

by an equal quantity of heat. The rise of temperature thus amounts to $0^{\circ}\cdot8$ R. or 1° C. Six seconds were required for the determination of the resistance. As, then, a rise of temperature of 1° C. augments the resistance 1 by 0.0005, thermal influence appears in fact not to be excluded.

These reflections I first made after having, as I believed, already proved, by numerous series of experiments without taking this circumstance into account, the dependence of the resistance upon the intensity of the current. Of the results of some of those series I will quote the mean values,—first, because at least so much can be inferred from them, that even after deducting the thermal alterations of resistance others still remain, and, secondly, because the comparison with later experiments, which were free from thermal influences, furnishes indeed a confirmation of the calculation we have just made.

1. Unannealed iron wire. $l=2120$, $d=0\cdot28$. In the mean of symmetrically distributed experiments in sets of 5 each:—

$$(1D) w_1=7\cdot7570, \quad (2D) w_2=7\cdot7600, \quad (3D) w_3=7\cdot7636;$$

$$w_2-w_1=0\cdot0030, \quad w_3-w_2=0\cdot0036.$$

Therefore $w_{n+1}-w_n$ is nearly constant, and

$$\frac{w_{n+1}-w_n}{w_n} = \delta = +0\cdot00043.$$

This number, it is true, is below the extreme value found for the effect of the heating; but the probable value corresponding in this case to the extreme value of that effect does not at any rate amount to more than the half of the former; it therefore amounts to no more than 0.00025. It is therefore established, to a high degree of probability, that *the resistance increases with the intensity of the current.*

2. Repetition of experiment 1.

$$w_1=7\cdot7482, \quad w_2=7\cdot7526, \quad w_3=7\cdot7574;$$

therefore mean of $\delta=0\cdot00059$.

3. As experiment 2 had given a higher value of δ than experiment 1, so a third series of experiments gave for it a still higher value; for the result was:—

$$w_1=7\cdot7469, \quad w_2=7\cdot7528, \quad w_3=7\cdot7587;$$

$$\delta=0\cdot00076.$$

This gradual increase of δ furnishes a confirmation of what was assumed at p. 219, that the high values of δ in the wire there under investigation might be a consequence of its having been several times used galvanically.

4. In a thin iron wire, Bunsen elements being employed, the following were found as the means of many experiments, of which two consecutive ones only were constantly used for the formation of the differences of resistance:—

$$w_2 - w_1 = 0.0037, \quad w_3 - w_2 = 0.0049, \quad \frac{w_3 - w_1}{2} = 0.0041,$$

Mean 0.0042;

(mean of $w = 6.83$) therefore

$$\delta = 0.00061.$$

If this δ were solely a consequence of the heating, it could not but be much greater in comparison with the δ of experiment-series 1.

The results of another series of experiments, in which the thermal influences were at most very slight, are graphically represented in fig. 7.

5. The share of the temperature-influences in the value of δ was compelled to represent itself isolatedly in a copper wire. The copper was pure, $l = 18000$, $d = 0.41$, and the mean value of $w = 4.164$. I found, on employing the same Bunsen elements as in 4,

$$w_3 - w_1 = 0.00090,$$

therefore

$$\frac{w_3 - w_1}{2w_1} = \delta = 0.00011.$$

This value agrees with the probable value of the thermal influence for this special case, so far as agreement is possible in rough calculations of this sort.

Of various artifices which I employed in order to exclude the influence of heating, the following finally proved to be the most effectual, at least with thin wires. The resistance of the iron wire was determined approximately, to one or two places of decimals. On closing the bridge, there then resulted a still smaller deflection toward one side, perhaps toward that to which corresponds too small a measuring resistance. The last figure of this measuring resistance was then made 1 higher by insertion in the resistance-case, so that now, on the closing of the bridge, a deflection resulted toward the opposite side. Let these two deflections be equal a_n and b_n for the case in which n elements are used; if these are small quantities (in the experiments their angles never exceeded $15'$), and if induction-phenomena of every sort are excluded, any observed deflection s_n can be reduced to an additive or subtractive resistance by dividing it by $a_n + b_n$, according as this deflection is observed at the taking-out or insertion of a unit in the last place of de-

cimals of the measuring resistance. In this way we can get with great exactness two more decimal-places; hence I made the experiments as follows:—

- (1) w approximately determined;
- (2) $s_1 s_2 \dots s_{n-1} s_n s_{n-1} \dots s_2 s_1$ observed, and from them a system of simultaneous values of $s_1 \dots s_n$ derived;
- (3) $a_1 + b_1, a_2 + b_2 \dots a_n + b_n$ observed;
- (4) Experiment 2 repeated inversely—

$$s_n s_{n-1} \dots s_2 s_1 s_2 \dots s_{n-1} s_n.$$

This again gives a system of simultaneous deflection-values. Finally the mean of these two was taken afresh. It then represents a system simultaneous with the determinations 3, and can by means of them be exactly reduced to a system of additive or subtractive resistances.

Here the current remained closed, on the average, only a few seconds; so that a thermal influence could hardly assert itself (compare below). On the other hand, the extra currents in thick wires made the procedure impossible; hence I was obliged to confine myself to thin ones.

6. Hard iron wire f_{10} . $l=24300$, $d=0.21$. 1–3 Daniell elements. (Preliminary experiment.)

$w=103.9$ deflections (to the left*):

$$s_1=0.0, \quad s_2=2.3, \quad s_3=4.7, \quad s_2=2.9, \quad s_1=1.2.$$

Therefore the simultaneous values are

$$s_1=0.6, \quad s_2=2.6, \quad s_3=4.7.$$

Now there were found

$$a_1 + b_1 = 4.7, \quad a_2 + b_2 = 8.6, \quad a_3 + b_3 = 12.2.$$

Therefore

$$w_1 = 103.913, \quad w_2 = 103.930, \quad w_3 = 103.939,$$

therefore in the mean

$$\frac{w_2 - w_1}{w_1} = \delta_{12} = 0.00017,$$

in fact much less than at p .

In the following Tables of the results of the exact experiments, the index in the first column states the number of the Daniell elements; the second column gives the sum $(a+b)$ of the deflections, left and right, for a unit more or less in the last decimal in the approximate statement of w ; s and s' are the mean values of the deflections found before and after the

* "To the left" denotes constantly "to the side corresponding to a too little measuring resistance."

determination of $a+b$; w and w' the corresponding true resistances; r denotes "right," l "left."

7. Wire f_{10} . From 1 to 6 Daniell elements. $w=103\cdot9$.

n .	$a+b$.	s .	s' .	w .	w' .	Mean.
1	4.6	0.5 r	0.5 r	103.889	103.889	103.889
2	8.5	0.1	0.1	899	899	899
3	12.2	1.0 l	1.0 l	908	908	908
4	15.2	2.3	2.9	915	919	917
5	17.6	6.0	6.1	934	935	934
6	20.0	9.8	10.1	949	950	950

$$\frac{w_2 - w_1}{w_1} = \delta_{12} = 0.00010.$$

Of the differences $w_n - w_{n-1}$ the first are constant—namely, 10, 9, and 9 respectively; the last two, on the contrary, are greater, namely 17 and 16: these latter point to thermal influences. These are in general easily recognizable in that they increase with n , while the influence in question here must diminish with n (on account of the gradual saturation of the wire).

8. Annealed iron wire f_{11} . $l=43200$, $d=0.205$, $w=182.6$.

n .	$a+b$.	s .	s' .	w .	w' .	Mean.	Difference.
1	1.6	0.6 l	0.8 l	182.638	182.650	182.644	26
2	2.9	1.9	2.2	665	676	670	5
3	4.4	3.1	3.6	668	682	675	3
4	5.8	4.0	5.1	669	687	678	9
5	7.1	5.6	6.7	679	694	687	15
6	8.3	8.2	8.7	699	705	702	

The influence of the heating is here still more clearly distinguishable from that of the intensity of the current. The latter gives

$$\frac{w_4 - w_1}{w_1} = \delta_{14} = 0.00019.$$

Hence we get $\delta_{12}=0.00014$. With the hard wire f_{10} the decrease of the quantities $w_2 - w_1$, $w_3 - w_2$, &c. was very slow. There, at all events, the influence of the current-intensity far exceeds $n=4$. But there $\delta_{14}=0.00028$, while $\delta_{12}=0.00010$. Therefore the dependence of the resistance on the intensity of the current is greater there than here; while here δ_{12} is greater, i. e. the dependence is concentrated upon a smaller region. If we imagine curves constructed for both cases, the abscissæ representing the current-intensities x , the ordinates the resistances

y , both will approach to the horizontal rectilineal form for x increasing; but this right line will possess a larger ordinate for hard wires, and commence at a larger abscissa. Compare fig. 8.

9. Repetition of 7. $w=102.4$.

n .	$a+b$.	s .	s' .	w .	w' .	Mean.	Difference.
1	3.4	2.0 r	2.9 r	102.341	102.315	102.328	
2	7.8	3.2	5.5	359	329	344	16
3	12.0	3.6	7.0	370	342	356	12
4	15.9	2.5	6.9	384	357	370	14
5	18.9	0.2	5.3	399	373	386	16
6	22.0	3.3 l	1.2	415	394	404	18

In all qualitative relations this series agrees with 7. The differences decrease very slowly; from $n=4$ they increase, in consequence of thermal influences; only the absolute values of the differences are somewhat greater; namely, there becomes

$$\delta_{12}=0.00016.$$

In order to fix for steel also the curves drawn for hard and soft iron, I made experiments with wires of elastic steel. They did not, however, in general yield results of corresponding trustworthiness. Only thus much could be concluded from them, that the ordinate of the straight horizontal line in which the resistance-curve with increasing intensity of the current ends is much greater than with hard iron, and commences at a still greater length of abscissa. I will communicate at least one such series of experiments.

10. Steel wire F_8 . $l=3500$, $d=0.43$. Approximately $w=3.53$ (for s) and $=3.54$ (for s').

n .	$a+b$.	s .	s' .	w .	w' .	Mean.	Difference.
1	8	3.7 l	5.8	3.5346	3.5328	3.5337	
2	14	8.3	9.3	59	35	47	10
3	19	11.6	8.6	61	55	58	11
4	24	13.5	7.6	? 56	68	? 62	? 4
5	28	19.9	7.6	71	73	72	10
6	31	25.8	7.0	83	77	80	8

Therefore

$$\delta_{12}=0.00033.$$

But the total alteration is not yet concluded, even at $n=6$.

The following last two Tables refer to control-experiments with copper wires. In both, for δ_{12} a very small negative value came out; at greater values of n the influence of the heating then asserted itself.

11. Wire of pure copper. $l=18000$, $d=0.41$, w (approximately) $=4.32$ and 4.31 respectively.

n .	$a+b$.	s .	s' .	w .	w' .	Mean.	Difference.
1	6.8	2.0 <i>r</i>	0.4 <i>l</i>	4.3171	4.3106	4.3138	-2
2	11.2	3.6	0.6	68	05	36	+2
3	15.0	4.7	1.0	69	07	38	0
4	19.7	6.1	1.3	69	07	38	1
5	24.0	7.0	1.6	71	07	39	1
6	27.8	7.7	2.1	72	08	40	

$$\delta_{12} = -0.00005.$$

12. Platinized copper wire. $l=15000$, $d=0.12$, $w=30.15$.

n .	$a+b$.	s .	s' .	w .	w' .	Mean.	Difference.
1	1.3	0.0	0.2 <i>l</i>	30.1500	30.1515	30.1507	-16
2	2.2	0.2 <i>r</i>	0.2 <i>r</i>	1491	1491	1491	+3
3	2.7	0.2	0.1	1493	1496	1494	+12
4	3.0	0.1 <i>l</i>	0.3 <i>l</i>	1503	1510	1506	26
5	3.2	0.9	1.1	1529	1534	1532	40
6	3.3	2.3	2.5	1569	1576	1572	

$$\delta_{12} = -0.00005.$$

I have shown that, when the intensity of the current rises from 1D onwards, the resistance also increases; and with soft iron wires we could follow this increase up to its limit. But now, how does the resistance change when, starting from 1D, we let the intensity fall? That then w also declines is indubitable; but two circumstances, further, favour the conclusion that it sinks rapidly, at least in soft iron wires. For, first, we have seen that the quantity $w_{n+1} - w_n$ diminishes when n increases, and therefore increases with a falling n ; and there is no ground for assuming that the curve changes its law at the arbitrary value $x=1D$. And, secondly, the magnetizing-experiments give us direct information respecting the total change of resistance with the circular magnetizing, consequently with the current-intensity; the negative values of δ , however, were disproportionately greater than the positive values which were here given for δ_{14} ; the rest must therefore arrive at the quantity δ_{01} . I have tried various methods for determining this quantity—that is, for comparing the resistance of an iron wire when the current is infinitely little with the resistance when the current has a finite intensity. They have, for the present, all failed, partly at the limit of sensitiveness of the galvanometer, partly at the comparison, which could not be carried out, even when the absolute determina-

tion of w_0 had been accomplished. I nevertheless intend to continue these trials.

The preceding considerations disclose to view an interesting analogy with some of those in dynamics and thermodynamics, which I will briefly express.

(1) The *coefficient of elasticity* is the ratio of an increment of pressure to the lessening of volume produced. Simultaneously with the latter, however, there results a rise of temperature. According as this is balanced or not by any force, *e. g.* by radiation or conduction, we obtain for the coefficient of elasticity a less or a greater value.

(2) *Specific heat* is the ratio of a quantity of heat to the rise of temperature produced. But simultaneously with the latter an expansion ensues. According as this is compensated by an external pressure or not, we get for the specific heat a less or a greater value.

(3) *The resistance of an electrical conductor is the ratio of an electromotive force to the current generated. But, if the conductor is magnetically polarizable, at the same time a circular magnetizing results. According as we balance this by any external force (for instance, by a longitudinal magnetizing) or not, we obtain for the resistance a less or a greater value.*

As the second proposition is reciprocal to the first, so a fourth can be placed over against the third; it does not, however, belong here.

Even Joule's law respecting the heating of the closed circuit cannot strictly hold good for iron; or, more exactly expressed, if in Joule's formula

$$W = \text{const. } i^2 w$$

we put, for the heat W developed and for the resistance w , values directly given by observation, we must obtain for the constant a value different according to the value of i , but always too high in comparison with other metals. Lenz has observed the times necessary for equal developments of heat with different current-intensities, resistances, and metals; he in fact found nearly constant numbers for the product ti^2w . These numbers, however, cannot be quite constant. For the theoretic deduction of the law presupposes the absolute constancy of the quantity w during the process under consideration; but in reality this never takes place, because the resistance is dependent on the temperature. It is only the electromotive force E that remains constant. Now, since Lenz's

constant can be written in the form

$$\frac{tE^2}{w},$$

it follows that every increase of w by the passage of the current must itself lessen that constant. Indeed Robinson* has confirmed this by showing that t diminishes with i increasing. But in iron, apart from the heating, a further increase of w takes place particularly through the magnetizing. Hence, with equal values of i , the constant must come out smaller for iron—indeed, since the true value to be taken into account is not

$$\frac{tE^2}{w},$$

but

$$\frac{tE^2}{w_0},$$

and since, according to the experiments in § 4, $\frac{w}{w_0}$ rises to 1.03, as much as 3 per cent. less. I have given at the commencement of this memoir (p. 3) the average numbers which result from Lenz's experiments. Their differences may most probably be attributed to errors of observation and the want of accuracy of the method. If, however, we try to bring the above reflections into harmony with Lenz's number for iron, this can be done perfectly; for indeed the number for iron is the smallest, and about 3 per cent. below the mean of the three other numbers.

The earlier view, that the temperature-coefficient α of the resistance of a metallic conductor might be the same for all pure metals, has not been verified by experience; *inter alia*, the deviations from the mean value 0.0037 are very considerable. Various quantities, too, must have an influence upon the value of α —for example, the specific heat, and, just as much, also the specific magnetism. As, according to G. Wiedemann†, the magnetizability increases with the temperature, and since, as we have shown, the resistance increases with the magnetizing by the current, *the resistance of iron must increase with the temperature more rapidly than that of other metals.* The value of α is, according to my data, in the mean about

$$\alpha = 0.0045.$$

This value is in fact higher than the mean value for the rest

* Trans. Roy. Irish Acad. xxii. (1) p. 3.

† Pogg. Ann. cxvii. p. 463; *Galvanismus* (2), ii. (1) p. 604.

of the metals, for which (with few exceptions) α lies between 0.0036 and 0.0038. In the supposition that a part of this difference is to be accounted for by the strong magnetism of iron we shall be confirmed when we consider that (with the exception of platinum) the lowest value of α belongs to bismuth, the most strongly diamagnetic metal—namely,

$$\alpha = 0.0035.$$

I have here selected only a few points, which show a relation to the question in hand in the most direct and striking manner. The reader who is intimate with the theories concerning it will find in other departments also, *e. g.* in the beautiful investigations of G. Wiedemann on the connexion between galvanic currents, torsion, and magnetism, multifarious points in which a connexion with the foregoing analyses is recognizable.

The carrying-out of my experiments in the laboratory of this University has been much furthered by the willing assistance of Professor Meyer, for which I here express my gratitude.

Breslau, 25th June, 1878.

XXVI. *Electro-optic Observations on various Liquids.* By JOHN KERR, LL.D., Free Church Training College, Glasgow.

[Continued from p. 102.]

31. **WE** come now to the fixed oils. Two of these (olive and castor) had been already examined carefully in the old cell—the former with good effect, the latter not. The fixed compensator is now withdrawn; but all the other arrangements are as formerly for CS_2 (6, 7). The principal difficulty of electro-optic experiments on liquids (the obtaining of a sufficiently clean charge) is now much aggravated. Sometimes the method of working already described (2) is not perfectly adequate, except at the expense of much time. Even when every known precaution has been taken, and charge after charge of the same oil passed through the cell, there are often a few visible specks left in the liquid, either solid particles or small bubbles, which interfere materially with the observations. But even in such cases, I generally get a pure and regular effect by taking a line of sight a little above or below the axial part of the field. And when the specks are very few, and the electro-optic action of the liquid not extremely feeble, the regular effect is often obtained at the centre of the field clearly enough, though it is always marred more or less by the presence of the solid particles or bubbles.

32. *Olive-oil*, the purest obtainable from the apothecaries—*Phil. Mag.* S. 5. Vol. 8. No. 48. *Sept.* 1879. R

transparent, and a very good insulator. As in all the insulating liquids yet examined, so here, electric force restores the light clearly from pure extinction in the polariscope. The effect is a good deal stronger than some of those already observed and certainly characterized. When a spark is drawn from the prime conductor, the light restored by electric force vanishes at once, though not with such apparently perfect abruptness as in CS_2 . When the light is well and steadily restored by electric action, and the hand compensator is applied in the usual way, horizontal compression is found to strengthen the light in every case, while horizontal tension weakens it to pure extinction. The action of olive-oil under electric force is therefore contrary to that of CS_2 (9), the one action being perfectly regular and (to sense) absolutely pure as the other.

33. *Oil of Sweet Almonds*, the best obtainable from the apothecaries—paler, and apparently purer and finer than olive-oil, and a very good insulator. The action of this liquid under electric force in the cell is not distinguishable from that of olive-oil, except that it appears rather stronger. Under good conditions easily obtained, the neutralization of electric action upon almond-oil by horizontal tension of glass is always perfect.

34. *Oil of Poppy-seed*, got in a good colour-shop as prepared for the use of artists—colourless, transparent, and a good insulator. I have never seen a perfectly clean charge of this oil in the cell; but I have obtained a regular effect with it in the way already described (31). The action of the liquid under electric force is similar to that of olive-oil, being always neutralized perfectly by horizontal tension of glass. I thought the effect somewhat fainter than those observed in the olive and almond oils.

35. *Oil of Rape-seed*.—As obtained from the oil-merchant, this liquid contained a large quantity of sediment. After half a dozen filtrations through Swedish paper, the oil was apparently clean and very clear, and of a faint amber colour. It acted in the cell as a moderately good insulator, the sparks from the prime conductor being reduced in length about two thirds when the connecting wires were put in position (10). In electro-optic experiments with this oil I never got a perfectly clean charge of the cell; but good optical effects were obtained with perfect regularity above and below the centre of the field. The effects were apparently a little fainter than those just observed in poppy-oil: they were equally pure, and of the same kind, the light restored by electric force from pure extinction being strengthened always by horizontal compression.

sion of glass, and weakened always to extinction by horizontal tension.

36. *Oil of Colza*.—As obtained from the merchant, this oil also contained a large amount of solid sediment. After several filtrations it was beautifully clear, and of a pale amber colour. Tried in the plate cell, it acted as a very good insulator, the sparks from the prime conductor being very little shortened when the connecting wires were put in position. In electro-optic experiment this oil was one of the cleanest that I had yet examined which accounts partly for the fine effects observed. These were of the same kind (contrary to CS_2) as in the preceding oils, but a great deal stronger. For the first time in any fixed oil, I now saw the extinction-bands well developed by the hand compensator. They were almost as fine as those formerly observed in cumol; and, as was to be expected, they afforded a good illustration of the difference between the fixed oils and the former liquids. It may be remembered that in cumol, as in carbon disulphide, the bands are developed and moved in towards the axis of the field, against electric force, by vertical tension of glass, or by horizontal compression. In colza, on the contrary, the bands are developed and moved in towards the axis by horizontal tension; and the action is pure and constant here as in cumol.

37. *Oil of Mustard-seed*, obtained as “Genuine East India” from the apothecaries—transparent, and of a rich yellow colour inclining to orange. Tested in the usual way, it acts as a pretty good insulator, the spark-length of the prime conductor being reduced not so much as one half when the connecting wires are placed. In electro-optic experiments with this oil I never got a perfectly clean charge; but good optical effects were easily obtained above and below the centre of the field. The effects were pure and perfectly regular, and of the same kind (contrary to CS_2) as in olive-oil, but apparently fainter.

38. *Raw Linseed-oil*, of a brownish-amber colour, fairly transparent, and only a moderately good insulator, the spark-length of the prime conductor being reduced by it about three fourths. This liquid was extremely troublesome in electro-optic experiment, chiefly from the difficulty of cleaning it. Not until many successive charges had been filtered and passed through the cell did the polariscope give any sure sign; and at the best the effects were obtained only well above or below the centre of the field. At last, however, the optical effect of electric force on this oil was found to be quite certain and regular, though extremely faint; and it was certainly of the same kind (contrary to CS_2) as in the other oils. A sample of refined linseed also was examined, and with similar results.

39. *Nut-oil*.—In the first electro-optic trial this oil was very dirty; but it gave a good regular effect, which was neutralized (as the effect in olive-oil) by horizontal tension. After several filtrations and renewals of charge in the cell the oil was very little improved, still dirty and hazy, giving a faint diffused light even at initial extinction, and showing a heavy chain between the electrified balls, as well as dancing particles in the outer parts of the field. But amid the confusion thus produced the optical effect was obtained regularly and clearly. The effect was of the same kind as in olive-oil, but apparently stronger.

40. *Lard-oil*.—As obtained from the oil-merchant this was merely a thick slush; but filtration gave a fine clear oil of a bright amber colour. Tested in the usual way, it acted as a pretty good insulator, the striking-distance of the prime conductor being reduced by it about one half. As with nut-oil, so here, I found that repeated filtrations and renewals of the charge made little difference. The oil was never perfectly clean: there were always a few flickering particles in the outer parts of the field, as well as a stationary chain or set of chains between the electrified balls. Still the effect of electric force was observed regularly and very distinctly in the polariscope. The effect was of the same kind (contrary to CS_2) as in olive-oil, but certainly stronger. The extinction-bands, though not in any case well developed, were brought out clearly by horizontal tension of the hand compensator.

41. *Neatsfoot-oil*, very like lard from first to last, but a purer-looking oil, and clearly stronger as an insulator. Under electric force it gave in the polariscope a perfectly regular and very good effect, which appeared stronger than that of lard. The effect was of the same kind as in all the preceding oils, being neutralized perfectly by glass extended in a direction parallel to the lines of force.

42. *Sperm-oil*.—Two samples were used, the purest that I could obtain from the oil merchants. They differed a little in colour, the one being a clear amber, and the other paler with a tinge of green; they differed also in insulating-power, the striking-distance of the prime conductor being reduced fully three fourths by one of them, and only about one half by the other. Both samples were examined at some length and with great care. Many successive charges were passed through the cell, the last of them very clean; but even at the best the liquid was somewhat hazy, as if from the presence of gelatinous matter. One of the samples, the weaker insulator, gave no sure trace of effect in the electro-optic experiment. The other sample gave an effect which was undoubtedly real, but too

faint to be characterized in the usual way. I therefore applied a more delicate method, which I have sometimes found very useful. Working from the best possible extinction, and before the electric force was applied, I introduced the hand compensator, and applied a very feeble strain steadily, so as to merely destroy the purity of initial extinction in one direction or the contrary. The electric force was then applied; and I found regularly that the effect of horizontal *tension* was clearly *strengthened* by electric force, and the effect of horizontal *compression* clearly weakened. These results were obtained consistently many times in succession, and with several successive charges; but, considering their singularity, I could not be satisfied till I had seen them under better conditions.

43. *Purified Sperm*.—Sufficient quantities of the two preceding samples were given to Mr. Tatlock, the public analyst for Glasgow; and he kindly had them purified for me with great care, by a process which is described in Miller's 'Chemistry.' As the oils left the hands of Mr. Tatlock he considered them extremely pure and dry. In electro-optic experiment there was now no difficulty or doubt about either sample, the effects being much stronger than before. By electric force the light was well restored from pure extinction; and the effect was always neutralized perfectly by horizontal compression of glass, and always strengthened by horizontal tension. Judging from memory, I thought the effect was at least as strong as the similar effect observed in Young's light paraffin (29), and a great deal stronger than the contrary effect observed in linseed-oil (38). One thing is certain, that the action of sperm-oil under electric force is of the same kind as that of carbon disulphide, and contrary in character to the action of every other fixed oil yet examined.

44. *Seal-oil*, a transparent but somewhat hazy liquid, of a pale amber colour, and a good insulator. In electro-optic experiment this oil acted as regularly as the others already mentioned, and gave excellent effects, though it was never perfectly clean. By electric force the light was restored brightly from extinction in the polariscope, and the effect was neutralized perfectly (as in olive-oil) by horizontal tension of the hand compensator. The extinction-bands were developed clearly, though not very strongly. The effects appeared to be the most intense that I had yet seen in the fixed oils excepting colza.

45. *Codliver-oil*, finest Norwegian, transparent, and of a faint greenish colour. In electro-optic experiment this oil gave regular and strong effects, which were neutralized perfectly (as in olive-oil) by tension of glass in a direction parallel to the lines of force. The extinction-bands also were seen dimly.

Elementary Measurements.

46. The only new piece introduced is a Thomson's long-range electrometer. My particular instrument is the second that Mr. White has constructed of the kind, and it is modelled closely on the first. For a full description and a good figure of this electrometer, I must refer to Sir W. Thomson's reprint of Papers on Electrostatics and Magnetism. The unit of scale-value of potential which I adopt is one turn of the screw; the whole range of the instrument is 200; and the highest potential ordinarily sustainable in the prime conductor of my plate-machine is about 180.

Three guarded wires are led from the prime conductor, one to the first outer ball of the plate-cell (the second outer ball being always connected with earth), another to the inductric plate of the electrometer, and the third to the knob of a small condensing-jar, whose outer coating is connected with earth. The use of the condenser is to slacken and regulate the rate of variation of the potential. The pieces and connecting wires are placed properly, so as to leave the action of the electrometer undisturbed. The rest of the arrangements are precisely as in the experiment of extinction-bands (11), and as exhibited in the diagram of 6.

47. *Method of Observation.*—The plane of polarization of the light rendered by the first Nicol being always at 45° to the lines of force or to the horizon, and the second Nicol being fixed at extinction, a weight of one or more pounds is attached to one of the plates of the fixed compensator, so as to give a definite initial restoration in the polariscope. Two observers attach themselves, one to the electrometer and the other to the polariscope; and the machine is worked at a proper rate, under direction from the first observer, so as to give a slow but steady rise of potential. The second observer watches for the first appearance of the patch of light at the centre of the black axial band (11), and he marks the instant of its appearance by a sharp signal. The first observer sees whether the index of the electrometer is beyond the sighted position or short of it at the instant of the signal, and he works the screw accordingly. The observation is repeated several times if necessary, the screw of the electrometer being properly worked each time until the index is in the sighted position at the instant of the signal; and the scale-reading of the electrometer is then recorded as the measure of the potential which neutralizes the definite birefringent action introduced at starting. Two other scale-readings are generally recorded at the

same time, one giving an observed lower limit of potential, and the other an observed higher limit: these are determined in the same way as the preceding, the index being clearly beyond the sighted position in the one case at the instant of the signal, and clearly short of it in the other case, always with rising potential.

Although the details of this method were adopted as the best that occurred to me in the course of a good deal of preliminary work, the final observations were not quite satisfactory. The effect in the polariscope varied in many cases so slowly that the observed limits of potential were too far apart. The compensator itself was subject to small irregularities, which it was almost impossible to prevent or to neutralize perfectly. Whether these difficulties are inseparable or not from the method, and how far they might be overcome by a good choice of materials and by very careful work, I am not yet able to judge.

48. *Carbon Disulphide*.—The following Table contains the results of two early sets of observations. Columns A and B give the numbers of pounds attached to the first and second plates respectively of the fixed compensator; column C gives the observed range of corresponding or sensibly neutralizing potential; D and E the observed lower and higher limits of potential; G the potential as inferred from the preceding numbers. Column K gives the observed increments of potential corresponding to a constant increment of birefringent action (2 pounds on second plate).

A.	B.	C.	D.	E.	G.	K.
1	0	57	56	58	57	
2	0	67	66	68	67	
4	0	86, 87	85	88	86½	
7	0	113, 115	112	116	114	
11	0	136, 138	135	139	137	
14	0	151, 154	150	155	152½	
2	0	70	69	71	70	21½
2	2	91, 92	90	93	91½	
7	0	112, 116	111	117	114	12½
7	2	124, 128	123	130	126½	
14	0	154	153	155	154	9½
14	2	163, 164	162	165	163½	

The next Table contains an unaltered record of the most of my last day's work with CS₂. All the conditions of observation were kept as constant as possible. The columns are designated exactly as in the former Table.

A.	B.	C.	D.	E.	G.	K.
1	0	55, 59	54	60	57	23½
1	2	80, 81	79	82	80½	
2	0	69	68	70	69	20½
2	2	79, 81	88	91	89½	
3	0	96, 98	78	82	80	17
3	2	89, 90	95½	98½	97	
4	0	103, 105	87½	91½	89½	14½
4	2	95, 98	102½	105½	104	
5	0	112, 116	94	99	96½	17½
5	2	104, 107	111	118	114	
6	0	119, 121	103	108	105½	14½
6	2	113, 115	118	122	120	
7	0		112	116	114	11
7	2	122, 123	124	126	125	
8	0	131, 132	120	124	122½	9
8	2	125, 128	130	133	131½	
9	0	136, 137	124	129	126½	10
9	2	130, 131	135	138	136½	
10	0	136, 139	129	132	130½	7
10	2	132, 136	135	140	137½	
11	0	140, 143	131	137	134	7½
11	2		139	144	141½	

49. I proceed to draw several inferences, beginning with an extension of the statement already made in 9.

(1) Throughout the range of these observations, the optical effects manifested are sensibly pure cases of double refraction; the plate of carbon disulphide acts, under electric force, as a positive uniaxal with axis parallel to the lines of force. This is evident from the nature of the arrangements, and from the constantly observed fact that, as the potential rises through a proper range of value, the light at the centre of the field passes through sensibly pure extinction.

(2) The total weight on the fixed compensator may be adopted as a good approximate measure of the corresponding birefringent action. The three following lines of numbers are taken from the second Table. The first line gives the total weights attached to the fixed compensator; the second gives the observed values of the neutralizing potential when the whole weight is attached to the first plate; the third gives the observed values of the potential when the weight is distributed between the two plates, 2 pounds always on the second plate.

3	4	5	6	7	8	9	10	11
80	89½	96½	105½	114	122½	126½	130½	134
80½	89½	97	104	114	120	125	131½	136½

From these results, and others of the same kind, I infer that, within the present range of measurement, the weight and the

optical effect are sensibly proportional, and each approximately a measure of the other.

(3) As the potential increases, the intensity of the corresponding birefringent action also increases. This is evident from both Tables.

(4) The increments of potential corresponding to a constant increment of birefringent action have sensibly smaller values at high than at low potentials. This is distinctly proved by column K of both Tables. It shows that the intensity of the optical effect varies more rapidly than the potential. Any conclusion more definite would be perhaps premature at present, and until the numerical results are more consistent; but the following is not without interest.

(5) Working from a fair average of the best measurements, putting v and v' for the scale-values of potential corresponding to two consecutive birefringent actions q and $q+1$ (measured by integer pounds of tension), and determining n successively so as to satisfy a set of equations of the form

$$\left(\frac{v'}{v}\right)^n = \frac{q+1}{q},$$

I find that the intensity of the optical effect varies more rapidly than the second power of the potential through the whole range of observation, but not more rapidly than the third power except for low potentials. Between birefringent actions 1 and 2, the value of n is slightly in excess of the number 3. The average value of n for all the observations is almost exactly equal to $\frac{5}{2}$.

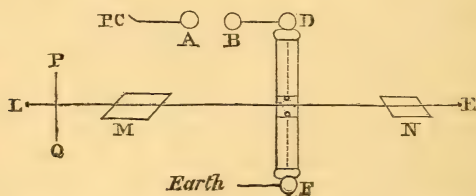
50. *Measurements in other Liquids.*—The method of observation has been already described in detail (47). The numbers in the first line of the following Table give the pounds of tension in the fixed compensator; those in the second line give the scale-values of the neutralizing potential in CS_2 , and so forward for the other liquids named.

	1	2	3	4
Carbon disulphide.....	57	69	80	90
Cumol	81	97½	111	122
Carbon dichloride	115	...	161	
Xylol.....	115	137	160	
Toluol }	130 ±	160 +		
Cymol }				
Benzol }				
Amylene	145 +			
Terebene	180 +			
Benzol (<i>bis</i>)	105	147	176	

The numbers for the first three liquids are probably exact ; those for xylol and the following liquids are doubtful, though certainly not far from the truth. It is worthy of notice that the potentials for carbon dichloride are to the optically equivalent potentials for carbon disulphide almost exactly as 2 to 1. The corresponding ratios for cumol as compared with carbon disulphide are also very nearly equal to each other, their values being 1.41, 1.41, 1.38, 1.35. We infer that optically equivalent potentials in carbon disulphide, cumol, carbon dichloride are very nearly as the numbers 10, 14, 20. The last line of numbers contains the results of a particular experiment (apparently a very accurate one) with benzol, in which the arrangements were so made that the restoration in the polariscope was in the form of a star or bright fringe on the contour of the positive ball. By a comparison of the numbers with those above them, it will be seen how great are the variations that may be induced in the optical effect by a small change of conditions, and how great is the care that must be taken to secure any thing like consistency of results in this line of work.

Optical Effect of Electric Discharge through Nitrobenzol.

51. *Experiment.*—The diagram shows all the pieces in horizontal section through the source of light L and the observer's eye E.



The cell D F is charged with nitrobenzol; and its outer balls D and F are connected by wires, one with the prime conductor and the other with earth. One of the wires is interrupted by an air-interval about half an inch long, limited by two brass balls A and B, which are supported upon movable pillars of varnished glass. The light from a paraffin flame L passes through a horizontal slit (about an eighth inch wide) in a screen P Q, then through the first Nicol M, which is fixed with its principal section at 45° to the horizon, then through the plate of nitrobenzol, then through the second Nicol N. The object seen in the polariscope is a narrow and very well outlined luminous oblong, lying along the axis of the electric field, and extending nearly from ball to ball.

Things being thus prepared, the second Nicol is turned to pure extinction, and the machine is set in motion. At the instant of each spark through A B, and then only, there is a clear instantaneous reappearance of the luminous oblong in the polariscope. As the interval A B is shortened while the machine is worked at a constant rate, the optical effect becomes fainter by degrees till it disappears, which it does before the air-interval is reduced to zero. The facts are exactly similar to those observed already in 24.

52. I have repeated this experiment with several variations; and I shall now mention the principal results.

(1) As long as the plate-machine is used, there is one thing found to be essential in all forms of the experiment: the discharging train from prime conductor through cell to earth must be interrupted by an air-interval (an insulating shell) A B of some length. The optical effect is observed at the instant of the spark through A B, and at that instant only. The position of the interval A B appears to be of no consequence; it may be before or after the cell, near earth, near cell, or near prime conductor. Mere discharge is not sufficient. Spark-discharge through a very short air-interval is, as we have already seen, without sensible effect; so also is glow-discharge, brush-discharge, and even crackling spark-discharge from the shaft of the insulated ball F upon the observer's knuckle or upon an earth-connected ball.

(2) When the prime conductor is connected with the knob of a small Leyden jar whose outer coating is connected with earth, the optical effect obtained as in 51 is very much intensified. And when the discharges through the liquid are obtained from a powerful Holtz (two 20-inch plates) which is provided with the usual condensers, the effect becomes still more brilliant. These observations agree with that already made (in 51) in connexion with the shortening of the interval A B.

(3) When the two Holtz conductors were connected with the outer balls D and F of the cell, the sparks through the air-interval A B in one of the connecting-wires became extremely dense and strong as A B was lengthened; and now, for the first time in my observations, the purity of the restoration in the polariscope was in some degree lost, the old object (though seen clearly enough) being displaced and distorted irregularly.

(4) Not even with the torrents of electricity rendered by this powerful machine when worked at the hardest, did the continuous discharge (air-interval zero) give any sensible effect from pure extinction in the polariscope.

(5) When a large induction-coil worked by one Grove's element was used as a source of electricity, I found it sufficient to connect the balls D and F with the two ends of the coil by unbroken wires. The light was then sustained continually from initial extinction in the polariscope, but always with a very sensible flicker. The liquid required in this case to be particularly clean; for otherwise the effect was marred by specks or chains of particles, as in the former experiments with insulating liquids when these were not very clean.

(6) The contrast between the central and the outer parts of the field, with reference to intensity of optical effect, is as marked here as in any of the insulating liquids. Returning to the experiment of 51, I remove the screen P Q and turn the flame L edgewise to the cell, so that the object in the polariscope is a narrow vertical streak of light extending well above and below the balls. The object now restored in the polariscope by discharge is a comparatively short central segment of the flame, lying well within the cylinder which envelops the two balls, and fading gradually to extinction at both ends, but very well outlined laterally.

53. *Experiment.*—The arrangements and procedure of 51 are retained with only one change; the plane of polarization of the light rendered by the first Nicol is horizontal or vertical—that is, either parallel or perpendicular to the line of discharge through the centre of the cell. The optical effect of discharge in the polariscope is now evanescent.

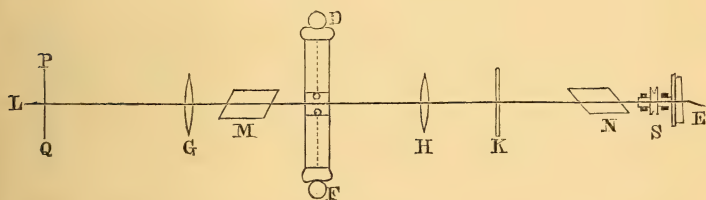
In most of my repetitions of this experiment, the restoration from pure extinction was reduced to an extremely faint but still perceptible flash at the instant of each spark; but when the cell was placed at a good distance from the eye, and great care was taken otherwise with the observation, this faint effect disappeared completely. The Nicols had only to be turned then through half a right angle each, to give the strong and clear effects already described in 51. These observations were made with the longest spark from the plate machine.

54. *Special Eyepiece.*—Many attempts were made to supplement the information obtained in the last article, by bringing the nitrobenzol-effect into closer comparison with the effects formerly observed in CS₂ and other insulating liquids. The results given by the hand compensator pointed certainly to one conclusion; but they were not very distinct, nor were they even regular enough to be satisfactory until recourse was had to a special eyepiece.

This is a small piece made of brass, and shaped somewhat like a common thread-bobbin with the shank prolonged a little way through one of the flanges, this flange being grooved as

a pulley. A very acute prism of flint-glass is laid flat upon the face of the other flange, and is attached rigidly to the bobbin by a screw cap, so that a ray of light, received along the axis of the tubular shank, enters the prism normally as it leaves the tube, and is then deviated through a small angle by refraction. The bobbin has its shank supported in proper bearings fixed at the edge of a whirling table, and can be made to revolve round its geometrical axis very rapidly. When a point is viewed through the bobbin by an eye placed close behind the prism, the rotation of the bobbin transforms the luminous point into a fine and faintly luminous circular line. With my particular prism, this ring of light is rather too large to be well commanded as a whole by the observer's eye; but a good view may always be had of as much as half the circumference. There is of course a little dispersion; but this is of no consequence.

55. *Arrangements.*—These are the same as in 51, with a few additions. All the optical pieces are shown in the adjacent diagram.



The flame L is turned broadside on to the cell; and the horizontal slit in the screen PQ is reduced to a very small width. The lens G is so placed as to give a real image of the slit at the centre of the cell, the image being a narrow and brilliant band, lying along the axis of the electric field, and extending from ball to ball. The second lens H assists the observer's vision. S is the special eyepiece (54), carefully placed beforehand so that the axis of the shank, which is also the axis of rotation, coincides with the line of the ray LMN. The piece K, between cell and analyzer, is a square plate of glass a quarter of an inch thick, which is permanently strained in a small screw press, the line of compression being parallel to the faces of the plate. The use of K is the same as that of the hand compensator; but its action is more uniform, and can be made very much stronger.

As in 51, the first Nicol M is fixed with its principal section at 45° to the horizon, and (the plate K being away) the second Nicol is fixed at extinction.

Let the plate K be now put in position as in the diagram, with the line of compression either vertical or horizontal. The light is restored in the polariscope, and appears (when the eyepiece S is at rest) as a sharply defined and very narrow horizontal band extending from ball to ball; but when the eyepiece is set in motion, the band is transformed into a large ring, narrow and bright at the top and bottom, wide and faintly luminous at the sides. The plate K is finally removed; and the extinction is seen to be pure.

56. *Experiment.*—Things being thus prepared, the cell is charged with clean nitrobenzol, and the balls D and F are connected with the terminals of a large Ruhmkorff's coil by unbroken wires. The coil is worked by one Grove's element, and gives a half-inch spark with broken circuit.

(1) The eyepiece S is left at rest. When the currents pass, the luminous band is restored continually in the polariscope, not, however, as a steady light, but flickering or quivering very rapidly. For our present purpose, this is a much better form of effect than any thing that can be obtained from the plate machine.

(2) The eyepiece S is set in motion at a moderate speed. When the currents pass, the light is restored in detached bands, which are distributed, of course, as generating elements of the luminous ring observed immediately above (55). No trace of the ring can be detected except the flashing bands themselves, these being separated by wide and perfectly dark intervals.

(3) Observation (2) is carried forward, the eyepiece being now driven round as rapidly as possible. There is a clear change of optical effect, the bands restored by the currents being enlarged into wide patches, which are much fainter in their later than in their earlier parts. It appears thus that each of the discharges of the coil through the liquid, considered as a discharge optically effective, occupies a sensible interval of time; it appears also that each discharge falls gradually in power towards its termination. There was nothing like this widening of the bands observed with the plate machine in any case, with or without condenser.

(4) The plate K is compressed, so as to give much the same effect in the polariscope as the hand compensator when well strained; and it is then put in position with the line of compression vertical (55). When the currents pass, and the eyepiece revolves at a moderate rate, the flashing bands are well restored on the faintly luminous ground of the ring; and when the eyepiece is driven very rapidly, the bands are widened as in (3). The flashes are quite as distinct to the eye as in

observations (2) and (3), though they are not now on a dark ground.

(5) Observation (4) is carried forward, with only one change: the plate K is laid with the line of compression horizontal. There is an evident change of optical effect. The flashing bands are fainter; and when the eyepiece revolves as rapidly as possible, the bands are not widened nearly so much as in (4). The optical effect of discharge appears thus to be weakened by horizontal compression.

(6) The compression of the plate K is increased by degrees, and the observations (4) and (5) are repeated at intervals. The contrast between the two cases comes out more and more strongly, till at last, when the compression has reached a certain value (a value greater than could have been anticipated), the contrariety is manifestly perfect. When the line of compression is vertical, the flashing bands are sensibly as distinct as ever; but when the line of compression is horizontal, they are evanescent and, under good conditions, quite imperceptible.

(7) When the right degree of compression has been reached by trial as in (6), the observer takes the piece K in his hands, and turns it rapidly round the ray, backwards and forwards through a right angle, the eyepiece revolving and the currents passing all the time. The effects are brought out thus in a very striking form, and are seen to be perfectly regular and decisive.

An intense electric current through nitrobenzol is therefore optically equivalent to tension along the line of discharge, or to compression in the perpendicular direction.

Summary.

57. The principal results will now be summed up briefly.

(1) When an insulating liquid is traversed by electrostatic force, it exerts a purely birefringent action upon transmitted light. In relation to this action, liquids are divisible into two classes, the positive and the negative.

(2) Positive liquids act as glass extended in a direction parallel to the lines of electric force, or as plates of quartz or other positive uniaxals with axes parallel to the lines of force. Carbon disulphide is the best example.

(3) Negative liquids act as glass compressed in a direction parallel to the lines of force, or as plates of Iceland spar or other negative uniaxals with axes parallel to the lines of force. Oil of colza is one of the best examples.

(4) In the following Table the positive liquids are arranged as nearly as possible in the descending order of electro-optic power, the larger and clearer intervals being marked by sepa-

rating lines. The negative liquids are not so arranged; but colza and seal oils are certainly among the strongest, and linseed is the weakest.

Positive Liquids.	Negative Liquids.
Carbon disulphide.	Fixed oils of vegetable origin:—
—————	Colza,
Cumol.	Sweet almonds,
Paraffin-oil (sp. gr. .890).	Olive,
—————	Poppy-seed,
Carbon dichloride.	Rape-seed,
Xylol.	Nut,
Toluol.	Mustard-seed,
Cymol.	Linseed.
Benzol.	Fixed oils of animal origin:—
—————	Seal,
Amylene.	Codliver,
—————	Lard,
Paraffin-oil (sp. gr. .814).	Neatsfoot.
Sperm-oil.	
Terebene.	
Bromtoluol.	
—————	
Valeric acid?	

The birefringent actions of these twenty-six dielectrics have been observed repeatedly; they are perfectly regular, and, to sense, perfectly pure. Valeric acid alone is so faint as to be doubtful.

(5) All the negative liquids yet known belong to the class of the fixed oils. Sperm-oil holds an exceptional place, being clearly positive.

(6) The influence of density on electro-optic power is marked and certain in the case of the paraffin oils, increase of density being accompanied by increase of electro-optic power.

(7) In carbon disulphide and several other liquids, electro-optic measurements are manageable through long ranges of potential and optical effect. The results of some careful trials in this direction are given in 49 and 50.

(8) Stannic chloride exerts a very strong optical action under electrostatic force; but the character of the effect is not yet certainly known (27).

(9) Of the forty or more liquids yet examined in the plate cell, there are none that exhibit any moderate degree of insulating power except the twenty-seven now named in (4), (8). This appears to justify the generality of the statement made in (1).

(10) When nitrobenzol is traversed by an intense electric current, it exerts a purely birefringent action on transmitted light. The action is similar to that of a positive uniaxal plate with axis parallel to the line of discharge.

58. In conclusion I shall give several reasons for preferring liquid dielectrics to solids in electro-optic experiment.

Liquids are free from temper and irregular strains ; and, if we except some such bodies as oil of turpentine, they are of themselves quite inactive in the polariscope. Solids, on the contrary, such as moderately thick plates of glass, are almost in every case irregularly active of themselves when inserted between two crossed Nicols. This difference is of much consequence when the work is delicate.

The optical effects of electric force are more complex in solids than in liquids, being partly due to electric force at the point or points viewed, and partly due to mechanical reactions of distant and differently strained parts of the body. I think it extremely probable that the remarkable phenomena lately observed by Mr. Gordon in plates of glass were due to these mechanical reactions.

Liquid dielectrics are not permanently damaged in any degree when they are traversed by disruptive discharge, while solids in such a case are rendered useless. And to see the practical importance of this difference, it should be noticed, *first*, that the preparation of a solid for accurate examination in electro-optic experiment is always a matter of some expense and trouble, and, *secondly*, that we cannot submit any dielectric thoroughly to such examination without running the risk of electric discharge through its mass.

When a plate cell has been carefully constructed once for all, any number of liquids may be examined in it successively, each liquid (if only clean enough) adapting itself perfectly to all the conditions of the experiment. Solid dielectrics, on the contrary, have to be drilled and mounted individually ; and this (as already said) is always a matter of expense and trouble.

The plate cell affords a geometrically constant field for any number of dielectrics in succession, which is a matter of capital importance in all comparative observations and measurements.

For the reasons now indicated, and notwithstanding the strength of glass and some other solids as dielectrics, I think it probable that liquid dielectrics will supersede solids altogether in this line of experiment.

XXVII. *On the Construction and Use of a Scale for Gauging Cylindrical Measures of Capacity.* By Sir G. B. AIRY, K.C.B., *Astronomer Royal*.*

SEVERAL years ago, when I was officially engaged on the national measures, I arranged a simple scale for rapidly gauging the cylindrical measures of capacity which are sanctioned by law, and which are in fact the only measures in ordinary use. I understand that this scale is now sometimes employed for its proposed purpose. I do not remember that the principle of its construction has been published; and I think that there may be advantage in making it known.

The cubical capacity of a cylinder, whose axial depth in inches is called "depth," and whose diameter in inches is called "diameter," is $= \text{depth} \times (\text{diameter})^2 \times \cdot 785398$. As the cubical capacity of the gallon is 277·2738 cubic inches, that of the half-gill (the smallest of our measures), or $\frac{1}{64}$ of the gallon, is 4·332403 cubic inches. Hence we have for the half-gill,

$$\text{depth}_1 \times (\text{diameter}_1)^2 \times \cdot 785398 = 4\cdot332403,$$

or
$$\text{depth}_1 \times (\text{diameter}_1)^2 = 5\cdot51619.$$

As the cubical capacities of our measures (half-gill, gill, half-pint, pint, quart, half-gallon, gallon, peck, half-bushel, bushel) proceed in continued binary progression, we have

$$\text{for gill,} \quad \text{depth}_2 \times (\text{diameter}_2)^2 = 2 \times 5\cdot51619;$$

$$\text{for half-pint,} \quad \text{depth}_3 \times (\text{diameter}_3)^2 = 4 \times 5\cdot51619;$$

and so on, the factors of 5·51619 being the successive powers of 2.

Taking the logarithms of both sides,

$$\log \text{depth}_1 + 2 \times \log \text{diameter}_1 = \cdot 74164,$$

$$\log \text{depth}_2 + 2 \times \log \text{diameter}_2 = \cdot 74164 + \cdot 30103,$$

$$\log \text{depth}_3 + 2 \times \log \text{diameter}_3 = \cdot 74164 + 2 \times \cdot 30103,$$

$$\log \text{depth}_4 + 2 \times \log \text{diameter}_4 = \cdot 74164 + 3 \times \cdot 30103;$$

and so on, the numbers on the second side increasing in arithmetical progression, with the common difference ·30103. We are at liberty to multiply these equations by any arbitrary

number; and we shall adopt the multiplier $\frac{10}{\cdot 3010300}$, or 33·21928. And we shall add to both sides the number

$$\frac{-10}{\cdot 3010300} \times \cdot 74164 + 10 = -14\cdot6368.$$

Thus we find, for half-gill,

$$33\cdot219 \times \log \text{depth}_1 + 66\cdot439 \times \log \text{diameter}_1 - 14\cdot6368 = 10;$$

* Communicated by the Author.

for gill,

$$33\cdot219 \times \log \text{depth}_2 + 66\cdot439 \times \log \text{diameter}_2 - 14\cdot6368 = 20;$$

for half-pint,

$$33\cdot219 \times \log \text{depth}_3 + 66\cdot439 \times \log \text{diameter}_3 - 14\cdot6368 = 30;$$

and so on, the right-hand number rising to 100 for the bushel. It will be remembered that the logarithms are common logarithms corresponding to the number of inches in the depth and in the diameter.

The number $-14\cdot6368$ may be divided into two parts, $-a$ and $-b$, subject only to the condition that $a+b=14\cdot6368$. And as we propose to use two engraved series of numbers, one relating to the depth and the other relating to the diameter, we may attach $-a$ to the first and $-b$ to the second; so that the numbers of the first column will be

$$33\cdot219 \times \log (\text{depth in inches}) - a,$$

and those of the second column will be

$$66\cdot439 \times \log (\text{diameter in inches}) - b.$$

And we have now to consider the details of the two engraved series which will represent these.

When we plunge the material scale into the cylinder, to touch its bottom, we obtain, in inch-measure on the scale, the quantity of "depth." But the engraved numeral is to give

$$"(33\cdot219 \times \log \text{depth}) - a,"$$

or

$$"(33\cdot219 \times \log \text{inch-measure}) - a."$$

Therefore

$$"33\cdot219 \times \log \text{inch-measure}" = " \text{engraved numeral} + a; "$$

and

$$" \log \text{inch-measure} " = \frac{\text{engraved numeral} + a}{33\cdot219};$$

and, taking the exponentials of both sides,

$$" \text{inch-measure} " =$$

$$\text{number whose log is } \frac{\text{engraved numeral} + a}{33\cdot219},$$

$$= \text{number whose log is } \cdot030103 \times (\text{engraved numeral} + a).$$

In like manner it is found for the scale which is applied to the diameter,

$$" \text{inch-measure} " =$$

$$\text{number whose log is } \cdot0150515 \times (\text{engraved numeral} + b).$$

By these formulæ the measures are given corresponding to every engraved numeral.

It will be remarked that the succession of engraved lines on these scales differs strikingly from that on the common logarithmic scale. With equal intervals of numerals, the intervals of engraved lines on the scale become larger with increasing numbers. The scales may properly be termed "exponential scales."

On referring to the investigation, it will be seen that, if the

depth of a cylinder be measured by the engraved numerals on the depth-scale, and the diameter by the engraved numerals on the diameter-scale, the sum of the measures will be 10 for half-gill, 20 for gill, 30 for half-pint, 40 for pint, &c.

As the zero-end of the scale is always to be brought to contact with the bottom or with the interior of the circle, that end ought to be hard; in other respects the scale may be a very light rod. If the bottom of the measure is spherical, the measure of depth to be adopted is the mean of those taken; one with the zero-end of the scale resting on the centre, and the other close to the circumference.

The following numbers have been computed for the graduations of the scales—using for *a*, 2·321, and for *b*, 12·315.

Measures, in Inches, for the Graduations of the Scale for
Depths.

Numeral.	Measure, in inches.	Numeral.	Measure, in inches.	Numeral.	Measure, in inches.	Numeral.	Measure, in inches.
2	1·349	24·8	6·551	32·4	11·094	37·4	15·690
3	1·446	25·0	6·643	32·6	11·249	37·5	15·799
4	1·550	25·2	6·735	32·8	11·406	37·6	15·909
5	1·661	25·4	6·829	33·0	11·566	37·7	16·020
6	1·780	25·6	6·925	33·2	11·728	37·8	16·131
7	1·908	25·8	7·022	33·4	11·891	37·9	16·244
8	2·045	26·0	7·120	33·6	12·057	38·0	16·357
9	2·191	26·2	7·219	33·8	12·225	38·1	16·471
10	2·349	26·4	7·320	34·0	12·396	38·2	16·586
11	2·517	26·6	7·422	34·2	12·569	38·3	16·701
12	2·698	26·8	7·526	34·4	12·744	38·4	16·817
13	2·892	27·0	7·631	34·6	12·922	38·5	16·934
14	3·099	27·2	7·737	34·8	13·103	38·6	17·051
15·0	3·321	27·4	7·845	35·0	13·286	38·7	17·169
15·5	3·438	27·6	7·954	35·1	13·378	38·8	17·289
16·0	3·560	27·8	8·065	35·2	13·471	38·9	17·410
16·5	3·685	28·0	8·178	35·3	13·565	39·0	17·531
17·0	3·815	28·2	8·292	35·4	13·659	39·1	17·653
17·5	3·950	28·4	8·408	35·5	13·754	39·2	17·775
18·0	4·089	28·6	8·525	35·6	13·849	39·3	17·898
18·5	4·233	28·8	8·644	35·7	13·945	39·4	18·022
19·0	4·383	29·0	8·765	35·8	14·042	39·5	18·147
19·5	4·537	29·2	8·887	35·9	14·141	39·6	18·274
20·0	4·697	29·4	9·011	36·0	14·240	39·7	18·401
20·5	4·863	29·6	9·137	36·1	14·339	39·8	18·529
21·0	5·034	29·8	9·265	36·2	14·438	39·9	18·658
21·5	5·212	30·0	9·395	36·3	14·538	40·0	18·789
22·0	5·396	30·2	9·526	36·4	14·639	40·1	18·920
22·5	5·586	30·4	9·658	36·5	14·741	40·2	19·051
23·0	5·783	30·6	9·792	36·6	14·843	40·3	19·183
23·2	5·863	30·8	9·929	36·7	14·946	40·4	19·316
23·4	5·945	31·0	10·069	36·8	15·050	40·5	19·450
23·6	6·028	31·2	10·209	36·9	15·156	40·6	19·586
23·8	6·112	31·4	10·351	37·0	15·262	40·7	19·723
24·0	6·198	31·6	10·496	37·1	15·368	40·8	19·860
24·2	6·284	31·8	10·643	37·2	15·475	40·9	19·998
24·4	6·372	32·0	10·792	37·3	15·582	41·0	20·137
24·6	6·461	32·2	10·942				

Measures, in Inches, for the Graduations of the Scale for
Diameters.

Numeral.	Measure, in inches.	Numeral.	Measure, in inches.	Numeral.	Measure, in inches.	Numeral.	Measure, in inches.
		38.0	5.720	57.2	11.127	66.2	15.199
2	1.643	38.5	5.820	57.4	11.204	66.4	15.304
3	1.700	39.0	5.921	57.6	11.282	66.6	15.411
4	1.760	39.5	6.025	57.8	11.360	66.8	15.519
5	1.822	40.0	6.130	58.0	11.439	67.0	15.627
6	1.887	40.5	6.237	58.2	11.519	67.2	15.735
7	1.953	41.0	6.346	58.4	11.599	67.4	15.844
8	2.022	41.5	6.457	58.6	11.679	67.6	15.954
9	2.094	42.0	6.570	58.8	11.761	67.8	16.066
10	2.167	42.5	6.685	59.0	11.843	68.0	16.178
11	2.244	43.0	6.802	59.2	11.925	68.2	16.290
12	2.323	43.5	6.921	59.4	12.008	68.4	16.403
13	2.405	44.0	7.042	59.6	12.091	68.6	16.517
14	2.490	44.5	7.165	59.8	12.175	68.8	16.632
15	2.577	45.0	7.290	60.0	12.260	69.0	16.748
16	2.668	45.5	7.417	60.2	12.345	69.2	16.864
17	2.762	46.0	7.547	60.4	12.431	69.4	16.981
18	2.860	46.5	7.679	60.6	12.518	69.6	17.100
19	2.961	47.0	7.813	60.8	12.605	69.8	17.219
20	3.065	47.5	7.950	61.0	12.693	70.0	17.339
21	3.173	48.0	8.089	61.2	12.781	70.2	17.459
22	3.285	48.5	8.230	61.4	12.870	70.4	17.580
23	3.401	49.0	8.374	61.6	12.959	70.6	17.703
24	3.521	49.5	8.420	61.8	13.049	70.8	17.826
25	3.645	50.0	8.669	62.0	13.140	71.0	17.950
26	3.774	50.5	8.821	62.2	13.231	71.2	18.075
27	3.907	51.0	8.975	62.4	13.323	71.4	18.200
28	4.044	51.5	9.132	62.6	13.416	71.6	18.327
29	4.187	52.0	9.292	62.8	13.510	71.8	18.455
30.0	4.335	52.5	9.454	63.0	13.604	72.0	18.583
30.5	4.411	53.0	9.619	63.2	13.698	72.2	18.712
31.0	4.488	53.5	9.787	63.4	13.793	72.4	18.842
31.5	4.566	54.0	9.959	63.6	13.889	72.6	18.973
32.0	4.646	54.5	10.133	63.8	13.986	72.8	19.105
32.5	4.727	55.0	10.310	64.0	14.083	73.0	19.238
33.0	4.810	55.2	10.381	64.2	14.181	73.2	19.372
33.5	4.894	55.4	10.453	64.4	14.280	73.4	19.507
34.0	4.979	55.6	10.526	64.6	14.379	73.6	19.642
34.5	5.066	55.8	10.599	64.8	14.479	73.8	19.779
35.0	5.155	56.0	10.673	65.0	14.580	74.0	19.917
35.5	5.246	56.2	10.747	65.2	14.681	74.2	20.055
36.0	5.337	56.4	10.822	65.4	14.783	74.4	20.194
36.5	5.430	56.6	10.897	65.6	14.886	74.6	20.335
37.0	5.525	56.8	10.973	65.8	14.990	74.8	20.477
37.5	5.622	57.0	11.050	66.0	15.094	75.0	20.619

And the rule for measuring the capacity of a cylinder, so far as to ascertain whether it agrees with the legal measures in ordinary use, is as follows:—

Measure the depth with the scale marked “*depth*” and take the reading.

Measure the diameter with the scale marked “*diameter*” and take the reading.

Add together the two readings. The sum will be :—

For half-gill	10·0
„ gill	20·0
„ half-pint	30·0
„ pint	40·0
„ quart	50·0
„ half-gallon	60·0
„ gallon	70·0
„ peck.....	80·0
„ half-bushel	90·0
„ bushel	100·0

XXVIII. *On a convenient Source of Heat for Chemical Operations.* By CHRISTOPHER CLARKE HUTCHINSON, Assoc. R.C.Sc.I. §c.*

MOST chemical laboratory operations require some source of heat, which should be capable of being readily applied and varied to meet the requirements of each individual case.

This is effected, in most instances, by the combustion of coal-gas in an ordinary Bunsen burner. Circumstances may, however, debar the operator from the use of coal-gas. He may be called upon to carry out some process, *e. g.* in the workings of a mine, at the side of a mineral spring, in the collection of water, gas, or air, in field blowpipe work, &c.; or finally, his laboratory may have to be hastily improvised in a country district (especially in a partially civilized country) where coal-gas is not obtainable.

He has then to fall back upon the older and more crude methods, such as the spirit-lamp, the wind- or charcoal-furnace, &c., which are at the best inconvenient, and often fail to give satisfactory results in their employment. None of them can be readily varied according to the desired temperature; and they require some dexterous manipulation, or a considerable amount of attention.

To supply this want, a simple method suggested itself to me which I believe may be of utility under some of the conditions above enumerated. It will be found efficient as well as simple, the cost both for material and apparatus being small, in addition to which it can be rendered exceedingly portable.

It is well known that many of the common forms of the liquid hydrocarbons are easily vaporizable, even at the ordi-

* Communicated by the Author.

nary temperature. Mixed with the amount of air necessary for their complete combustion, it occurred to me, their vapours would form an efficient substitute for coal-gas. This method I have tried, and found to yield excellent results.

The vaporization is effected by passing a brisk current of air through a column of the liquid, sufficient being vaporized to yield an exceedingly efficient combustible mixture.

The apparatus used is exceedingly simple and of trifling cost, the liquid hydrocarbons used being either common methylated ether, benzol, naphtha, or common alcohol. The mixture of the benzol hydrocarbons, almost universally sold and known as "*benzoline*," is preferable, on account of its small cost and its higher volatility.

A moderately tall and rather narrow cylinder (like those used in the Wanklyn process of water-analysis, or even a large test- or boiling-tube) is fitted with a good cork, through which pass two tubes analogous to those of an ordinary wash-bottle: one passes to the bottom of the cylinder and serves as an ingress-tube for the current of air, the other (just entering through the cork) serving for an egress-tube. The cylinder being almost filled with the volatile liquid, a brisk current of air is passed through, and the air, saturated with vapour, led off from the egress-tube to an ordinary Bunsen burner, where a steady and intensely hot flame is obtained. By regulating the current of air or vapour by means of a tap, the flame can be varied with the same facility as that of an ordinary gas-flame.

The current of air may be maintained by means of a large gas-holder, a reversible aspirator like that used by Dr. Angus Smith, or even a foot-bellows. By fitting up a large test-tube in the manner described, and by using a small foot-bellows, an exceedingly portable apparatus is obtained. For the supply of a small laboratory where a number of lights are required, the apparatus can be varied with equal facility and at very trifling expense. The vaporizing-cylinder may be replaced by a metallic vessel, or a tall, somewhat narrow bottle; and a continuous and powerful supply of air can be maintained from the apparatus described by P. Casamajor* in the '*American Chemist*.' This apparatus, being worked by the supply of water from one of the service water-taps, needs scarcely any attention; and thus a continuous supply of combustible vapour can be maintained. The egress-tube from the cylinder must be furnished with branch pipes or T-pieces, as an ordinary gas-pipe.

The combustion of these vapours, it will be observed, gives

* '*American Chemist*,' vol. iv. p. 361.

even a higher temperature than that of coal-gas; they also possess the advantage of yielding pure and innocuous combustion products—water and carbonic anhydride. They can therefore be employed without danger in the vicinity of delicate instruments, which would be otherwise injured by the acid combustion-products of our impure gas supplies. All who are in the habit of using platinum vessels are aware of the injurious effect produced upon their surfaces submitted to the action of the flame of impure gas: with the use of these flames this effect is obviated, and delicate platinum surfaces and instruments can be heated or cleansed in them without injury.

Experiments made to determine the cost for the supply of an ordinary Bunsen-flame gave the consumption of common ether about 1.5 ounce per hour, the cost of which is about two pence; with common benzoline the consumption was at the rate of one ounce per hour, or under one farthing.

In tropical climates, such as that of India, where the great volatility of ether or benzoline would be unfavourable to their use, they might be replaced by naphtha or common alcohol. Filling the vaporizing-cylinder with naphtha and immersing in water at about 120° F., an intensely hot flame was maintained from the resulting vapour. With alcohol the same result followed when immersed in water at about 130° F. In those countries where the operator is deprived of the use of coal-gas this method for obtaining a supply of heat could be used with advantage.

The experiments were made in the laboratory of the Royal College of Science, under Professor Galloway.

XXIX. *Notices respecting New Books.*

The Art of Scientific Discovery, or the general conditions and methods of research in Physics and Chemistry. By G. GORE, LL.D., F.R.S. London: Longmans, Green, and Co. 1878. (Crown 8vo, pp. 648.)

THIS is the work of a well-known chemist, of one who has practised the art of discovery, and who might be expected to discourse well on an art in which he has obtained a certain measure of distinction. Nor is the book by any means devoid of interest; indeed, a partial reviewer might easily point to many well written and instructive passages, he might allege that the book is a store-house of facts, that its contents show not only that the author is well acquainted with some branches of science, but also that he has read the history and at least paid attention to the philosophy of science. And yet, while recognizing the truth of all this, we must confess that we have read the book with great disappointment; it seems to us to raise expectations which it by no means fulfils,

and suggests comparisons which are very damaging. While reading it, we involuntarily call to mind the "Discourse on the Study of Natural Philosophy," and how, in the one, every thing is clear and the scope and purpose of the whole plain, while in the other it is hard to see the object the author has in view, and the parts, though often interesting when considered as fragments, are apt to have no manifest bearing on the matter in hand.

The book is on the Art of Scientific Discovery; and, of course, the question arises whether there be such an art. One way of answering the question would be to allege that there is an art of discovery in much the same sense that there is an art of war, or an art of poetry—that it is possible to explain the methods by which particular discoveries were made, to analyze the evidence which is sufficient to distinguish ascertained truth from hypothesis, to put in order, and render easy of acquisition, the positive knowledge hitherto acquired, to indicate some of the directions in which those who seek are likely to find new truths, but that where an advance is to be made the matter must be left to the patience and sagacity of the well-informed inquirer. The question has, of course, been answered in other ways. Lord Bacon hoped that his method, which, however, he never completely expounded, would do a great deal more:—"Our method of scientific investigation is such as to leave little to strength and keenness of intellect, and to reduce all understandings nearly to a level. When it is required that a line be drawn straight or a circle round, much depends on the steadiness and practice of the hand, if it is to be done by the unassisted hand, but little, or nothing, if a ruler is used, or a pair of compasses; our method is just like this" (Nov. Org. i. 61). This seems quite plain as to what Lord Bacon hoped for; but it is certain that his expectation has not been fulfilled. We, however, are concerned here with Dr. Gore's opinion; but we find it hard to say what that is. We learn that "the late Mr. Faraday expressed" a "favourable opinion of my proposition of framing an art of scientific discovery;" so that one would have thought that his expectation resembled Bacon's; but on the same page we learn that he has not forgotten, and apparently allows the justice of, Dr. Whewell's "assertion that, speaking with strictness, an *art* of discovery is not possible," and that he has no wish "even to suggest the idea of reducing all intellects to a level." His "purpose is only to show that an art of scientific discovery is much more possible than it was in the time of Lord Bacon" (pp. vii, viii). Dr. Gore's notion, then, appears to be that to form an art of discovery is not possible, yet it is more possible than it was formerly; and meanwhile he has undertaken to write "about it, and about it."

This indeterminateness as to the purpose of the book characterizes its parts. Let the reader take for instance Chap. xxviii., "Circumstances and occupations favourable to scientific research," and ask himself what is the point that the author wishes to establish: we doubt whether he will be able to answer the question. The circumstances referred to, taken in order, we should enumerate thus:—Professions; wealth or poverty; education; encouragement at the

hands of government; suitability of epoch; sympathy of others; persecution; employment as teacher, lecturer, or examiner; acting as assistant to other investigators; age; marriage; connexion with scientific societies, and "international encouragement." It is plain that many of these circumstances may be unfavourable as well as favourable to scientific research; and that is how Dr. Gore treats them; but putting that on one side, we can scarcely be wrong in supposing that these "circumstances" were treated of just as they occurred to him. There is no apparent attempt at classification, and no reason why the list should not be made longer: *e.g.* why not include amongst the "circumstances" the character of the philosopher's father, or of his mother, whether he was an only child or had brothers and sisters, whether he was a teetotaller or given to wine and strong drinks, and so on? The fact is that there is no end to such a list; every circumstance of a man's life may be looked at in such a way as to suggest the question, Is it favourable to scientific research? But there are only a few circumstances in which this point of view is not very artificial; and those few Dr. Gore has neither singled out nor discussed. A careful and discriminating account of the influence exercised by such "circumstances" as encouragement at the hands of Government, connexion with a scientific society, or active employment as a teacher, would have been instructive and pertinent; and this he has hardly attempted. But when it comes to the influence of marriage on scientific research, the wrong question is raised. In most cases, it is true, a man can only support a wife and family by spending all his strength on his profession; and, of course, absorption in a laborious profession is inconsistent with scientific research. In these cases, marriage is simply incompatible with research. If, however, we suppose this urgent necessity not to exist, the question is then, not what the influence of marriage, but what the influence of the woman married will be. If the philosopher, for example, marries X, she will do her best to keep him to his crucibles; but if Y, she will make him frequent kettledrums and other scenes of laborious amusement. The influence of marriage on research, it would seem, cannot be discussed without a previous classification of the characters of women; and this, it is needless to say, Dr. Gore does not attempt. To tell us that Lady Hamilton helped her husband to write out his lectures (p. 280), that Mrs. Flaxman was very much annoyed with Sir Joshua Reynolds for telling her husband that by marriage he was ruined for an artist (p. 281), throws little light on the subject, not much more than comes from the luminous fact that twenty thousand men and women went to see Buffon buried (p. 263). Oddly enough, too, Dr. Gore groups his examples as if on purpose to show that men become active in scientific research independently of these "circumstances:" thus, some eminent men of science have been rich, others poor; some have been well educated, others the reverse; some have enjoyed the patronage of their sovereign, which others have been entirely without, and so on. The only thing that can be said appears to be that extreme poverty, absolute want of edu-

education, total absorption in a laborious profession, are obstacles not to be overcome; but as this is quite plain to begin with, the whole discussion is singularly wanting in purpose.

It is sometimes curious to notice the irrelevancy of the details of Dr. Gore's illustrations; thus *à propos* of poor men becoming eminent for research, he mentions Hunter, who began life as an apprentice to a cabinet-maker, in which capacity he, naturally enough, "constructed chairs and tables" (p. 263). He afterwards went into surgery and made the famous collection of anatomical preparations. His case affords a striking instance of genius and industry overcoming obstacles, and proves conclusively that poverty in youth is not inconsistent with a life devoted to scientific research. Dr. Gore's way of telling the story is this:—"With the proceeds of his practice as a surgeon, he bought all the bodies of the wild beasts that died in the Tower of London, and of every other such animal as he could procure, and dissected them; he compared the anatomy of them all, and discovered the history of their organs. In this way he expended more than £70,000 in money, besides immense labour. He was in the habit of swallowing thirty drops of laudanum before delivering each lecture. He died at the age of 80; and after his death the English Government gave £15,000 for his collection of about 20,000 anatomical preparations" (p. 264). This might be an illustration of the fact that poverty in youth is not inconsistent with the expenditure of large sums of money in middle life; but the curious point is the "thirty drops of laudanum:" the fact is interesting, but utterly irrelevant; for Dr. Gore has not the least intention of showing that taking narcotics is a circumstance favourable to scientific research. Nor is this a solitary instance: on the preceding page (263) the facts about Linnæus are nearly all irrelevant. That Linnæus was not strong, but lived to the age of 71 years, that his collection of plants and insects was sold after his death for £1000, and that the King of Sweden sent a ship of war in chase of the ship that was taking it to England are facts which have nothing to do with the matter in hand; but Dr. Gore happened to know them, and so wrote them down. We had marked several other passages for comment; but we have perhaps written enough to show that the disappointment produced by the perusal of the book was not without reason, and to justify our opinion that, in spite of his possessing several qualifications for the task, Dr. Gore has thrown little or no light on the Art of Discovery.

Wheatstone's Scientific Papers. Published by the Physical Society of London, 1879.

The Physical Society of London is to be thanked for having undertaken, and to be congratulated for having brought to so successful an issue, the republication, in a collected form, of Sir Charles Wheatstone's published researches.

Wheatstone's contributions to Physical Science need not be looked at through the same glass which is used to magnify the merits of many so-called scientific publications. He does not seem

to have written unless he had some discovery of his own to announce. Such announcements were made in language, if not always the most polished, yet certainly, to scientific minds, the most pleasing, because the most simple. On looking through his papers here collected, it seems that almost all his discoveries were crucial.

He was essentially witty, if by wit is meant nimbleness in correlating divers impressions. By viewing an idea from various sides and combining those views, he produced a discovery, a solid fact.

He never wrote for the sake of writing; nor did he regard physics as the application of mathematics to hypothetical matter. His mind was in unison with Nature. He seemed to feel that theorization should be the servant, not the master, of investigation; and hence his superiority in usefulness over most of his contemporaries.

The publication of this collection of memoirs by the Physical Society, in addition to its own 'Proceedings,' shows, as did the publication of Prof. Everett's C.G.S. system of physical data, that the energies of the Society are well directed. It is to be hoped that the scattered memoirs of many another physicist may be collected and published by the same means.

XXX. *Intelligence and Miscellaneous Articles.*

THE ACTION OF MAGNETISM IN MOTION ON STATIC ELECTRICITY;
INERTIA OF STATIC ELECTRICITY. BY G. LIPPMANN.

IT is known that previously to Ørsted's experiment it was in vain attempted to connect magnetism with static electricity. Now-a-days we can be certain that such a relation really exists, and we can formulate the law of it even before making the experiment. A magnet in motion exerts at a distance a mechanical action upon a body at rest charged with free electricity. This action strictly results from the existence of the inverse phenomenon, which has been established by the experiments of Mr. Rowland*.

It will be remembered that Rowland showed experimentally that the motion of an electrized body acts on the magnetized needle as a current would: the direction of the action changes with the sign of the electric charge; and the action due to a given displacement of electricity is the same as if that displacement had taken place under the form of a current properly so called. Such are the results obtained by Mr. Rowland. On this ground I say that Mr. Rowland's phenomenon is necessarily reversible, and that its reversibility is a consequence of the impossibility of perpetual motion.

In fact, if we displace an electrized body so that each point in it describes a closed trajectory n times, resuming every time the same velocity at the same point, a magnetized needle near it is submitted during the movement to periodically variable forces, in virtue of Rowland's effect; this needle can therefore move under the action of those forces, and furnish a quantity of work which differs from

* Helmholtz, *Berl. Bericht*, 1876 (*Journal de Phys.* t. vi. p. 29); *Phil. Mag.* [5] vol. ii. p. 233.

zero, although it takes up again at each period its initial position. The work is not *nil*, because the forces producing it depend at each instant on the velocity of the electrized body, and not merely on its position. Therefore the magnetized needle furnishes a finite quantity of work, which becomes infinite with the number of the periods. Besides as the system travels along a closed cycle, this work is derived entirely from the forces which maintain the motion of the electrized body. That body is therefore itself submitted to resistant forces, which depend on the velocity of the magnetized needle. It is the existence of these last forces that we wish to demonstrate. If Rowland's effect is the analogue and as it were the complement of the phenomenon discovered by Ørsted, the inverse phenomenon here signalized corresponds in the same manner to induction. It is even found that moving a magnetic field produces, upon a small body charged with a unit of electricity, a mechanical force equal in amount and direction to the electromotive force at the same place; only we have here not an electromotive force without action on the masses, but a force properly so called.

From the preceding a curious consequence may be deduced; it is, that static electricity possesses a proper mechanical inertia, which is simply added to that of the electrized body. If, indeed, an electrized body is in motion in a space where there is no magnet, this motion gives rise to a magnetic field, since a magnetic needle in its vicinity would be deflected. The intensity of the magnetic field is proportional to the velocity; and consequently the variation of that intensity is proportional to the acceleration of the body. Now, from what we have seen above, the variation of a magnetic field produces upon an electrized point a mechanical force equal to the electromotive force of induction, consequently proportional to the velocity of the magnetic variation, and therefore to the acceleration of the body, and with the same direction as the acceleration. But a mechanical force directed thus, and proportional to the acceleration, constitutes what is called a *force of inertia*.

The ratio of the force to the acceleration is a constant quantity for the same electric charge, but not simply proportional to the quantity of electricity.—*Comptes Rendus de l'Académie des Sciences*, July 21, 1879, t. lxxxix. pp. 151-153.

ON THE SENSIBILITY OF THE ORGAN OF HEARING.

BY W. KOHLRAUSCH.

With the aid of a toothed wheel working against a disk of metal or pasteboard, Savart* found that two impulses can make upon the ear the impression of a comparable tone; M. E. Exner† finds by means of tuning-forks vibrating before spherical resonators that 17 impulses, M. Pfaundler‡ again, by experiments on holed sirens and reflection-tones, that only two impulses are required; lastly, M. Auerbach§, nearly in accordance with M. Exner, that about 20 vibra-

* *Ann. de Chim. et Phys.* xlv. p. 348 (1830).

† Pflüger's *Archiv*, xiii. p. 228 (1876).

‡ *Wien. Ber.* lxxvi. p. 572 (1877); cf. A. Seebeck, *Pogg. Ann.* liii. p. 417 (1841).

§ *Wied. Ann.* vi. p. 591 (1879).

tions are necessary for the production of a tone in the physiological sense, the tone being determined to within the interval 100 : 101.

Less sharply defined tones can be produced, in an extremely simple manner, by only two impulses. We have only to put two fingers of the hand together so that the ends of the nails are level, and then strike somewhat obliquely on a table, for instance, whose proper tone is as much deadened as possible by putting books upon it, a closed chest of drawers, &c., eventually moreover by firmly supporting our own body upon it. We shall readily feel, in our hand, that the two fingers but rarely strike it simultaneously; and with a little attention we always hear (best when the stroke is repeated from twice to three times in a second), together with the noise (undetermined as to height) of the knocking, a very hollow tone and of a height which changes *per saltum* according to the position of the fingers, but which by practice we can approximately have at our command. Similar tones are obtained by running the finger-nail over short lengths of ribbed paper.

That real tones are heard on knocking can most easily be perceived from the fact that, on knocking twice with the indicated difference of time, one can almost always state the interval of the tones, and indeed can imitate them (most easily in a whisper), if it approximates to one of the musical intervals to which we are accustomed. I have often heard with certainty differences of height of tone to within about a semitone. The tones from only two impulses are therefore determined with certainty to within the interval 15 : 16.

If we strike with only one finger-nail, these tones are entirely lost; hence we can easily learn to hear by this if the knocking takes place alternately with one and with two fingers. From another side also their presence and the correct determination of the heights and interval of the tones was confirmed to me.

Now the employment of resonators shows indeed that in the noise produced by one knocking on wood, pasteboard, or metal disks whose proper tones are deadened, almost all heights of tone are represented, but so feebly that, in presence of the distinctness of our tones, a resonance in itself unlikely of the objects struck may be unnoticed during the period of the observed tone in question. With a perfectly quiet surrounding, the tones produced by the faintest knocking on stone walls and other heavy solid objects can be heard, in which materials such an aftervibration in consequence of the two feeble shocks, and consequently an objection against the assertion that the tones are rendered perceptible by only two impulses, is certainly excluded.—Wiedemann's *Annalen*, 1879, No. 6, vol. vii. pp. 335, 336.

ON A DIRECT MEASUREMENT OF THE WORK OF INDUCTION,
AND A THENCE-DERIVED DETERMINATION OF THE MECHANICAL
EQUIVALENT OF HEAT. BY DR. A. VON WALTENHOFEN,
OF PRAGUE.

The author has made experiments for the purpose of measuring

directly, by means of a spring dynamometer, the work required for the induction of an electric current of determined strength in a circuit of given resistance, and comparing it with that calculated from theory.

A magneto-electrical machine for continuous currents served for the induction-apparatus, the electromotive force of which had previously been exactly ascertained and found to be proportional to the number of turns. The dynamometer was a dynamometric winch of the newest construction, provided with a marking-apparatus for sketching the work-curves. Its scale was tested by direct loading, and found accurate. The dynamometric winch was screwed to the induction apparatus on an axle attached to the machinery instead of the ordinary winch.

For the measurement of the induced currents a tangent-compass, the reduction-factor of which was exactly ascertained, was inserted in the circuit, of which the resistance was measured as accurately as possible, and could be altered at pleasure by means of inserted scales. For counting the number of turns a seconds' pendulum with a loud stroke was used.

Five experiments were made: in three the velocity of rotation amounted to 1 revolution of the winch in 1 second (corresponding to 7 revolutions of the inductor); in the two other experiments 1 turn of the winch took 2 and 4 seconds respectively. In each experiment 65 turns were executed—once with interrupted, and once with closed circuit. The difference between the work recorded by the dynamometer in the one case and in the other was the work of induction expended for the production of the current simultaneously measured on the tangent-compass according to the proportion of the electromotive force calculated from the number of turns or from the known resistance. It amounted, according to the very well accordant results of the five experiments, in which the expenditure of induction-work lay between the limits of $\frac{1}{3}$ and 6 meter-kilograms, to the electromotive force of one Daniell element, and to the resistance of one Siemens unit, reduced 0.13 metre-kilogram per second—a result which comes very near theoretical determinations.

Comparing this value of the work with the number of calories corresponding to the chemical processes that take place in a Daniell series with equal resistance, we get for the mechanical equivalent of the heat, on using the numbers given by W. Thomson and Jenkin, the number 428, or, if we take as the basis of the calculation the higher amount of induction-work in the first four experiments, the number 421, very closely agreeing with the generally accepted equivalent of Joule.—*Kaiserliche Akademie der Wissenschaften in Wien, mathematisch-naturwissenschaftliche Classe*, July 3, 1879.

ON THE RADIOMETER. BY DR. J. PULJ.

The paper contains a criticism of the evaporation theory of Osborne Reynolds and the emission theory of Zöllner.

Were evaporation or the emission of molecules on the irradiated side of the vane the only or at least the chief cause of the radiometer-motion, this would of necessity *increase* when the rarefaction is continued, since according to experience both evaporation and the emission of molecules must be so much the more vigorous the lower the pressure in the space occupied by rarefied gas. But, as we have learned from experiments by Finkener and Crooks, the moment of rotation exerted by the flame upon the radiometer at first increases, *cæteris paribus*, with the rarefaction of the gases, reaches its maximum at a certain pressure, and with further rarefaction *decreases*. This diminution of the radiometric effect, which may fall to $\frac{1}{10}$ of the maximum value, contradicts the above-mentioned consequence of the theories of evaporation and emission. On the other hand, however, the hypothesis that all bodies, independently of their aggregate-condition, evaporate even at ordinary temperatures cannot be called in question; hence it must be admitted that a force of reaction will also result from the emission of molecules. But, in order to explain the above-mentioned diminution of the radiometric motion, it must be assumed, further, that the reaction-forces resulting from the emission must be either vanishingly small, or so small in comparison with the forces arising from the recoil of the molecules of the gaseous material still present that the motion-phenomena are either exclusively or chiefly conditioned by the latter forces.

On this hypothesis, that decrease of the motion can be explained, according to the kinetic theory of gases, in the following manner:—At the full atmospheric pressure the reaction-force called forth on the irradiated side of the vane is too little to overcome the resistance presented by friction and the air. At sufficient rarefaction the reaction-force overcomes those resistances, and the motion of the vane commences. If the reaction-force, like the internal friction, decreases only very slowly with the pressure, the velocity of the movement attains its maximum at a certain pressure, and decreases when the rarefaction is continued, because not merely the resistance of the air, but also the force of the reaction excited becomes less with the lessened number of the recoiling molecules. In a space absolutely void of air the motion must cease entirely, if no emission of molecules results on the irradiated side of the vane.

Further, the author describes a radiometer consisting of a fixed cross of mica lamellæ blackened on one side, and a very thin cylindrical glass case. The distance of the outermost edges of the vanes from the glass amounted to 2 millims. When light fell upon it the glass cylinder rotated in the opposite direction to that in which the cross would have been caused to turn.

The object of this experiment is, to furnish a complete demonstration that the motions in the radiometer cannot be explained by means of currents of air.—*Sitzung der mathematisch-naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften in Wien, July 3, 1879.*

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

OCTOBER 1879.

XXXI. *Investigations in Optics, with special reference to the Spectroscope.* By LORD RAYLEIGH, F.R.S.*

[Plate VII.]

§ 1. *Resolving, or Separating, Power of Optical Instruments.*

ACCORDING to the principles of common optics, there is no limit to resolving-power, nor any reason why an object, sufficiently well lighted, should be better seen with a large telescope than with a small one. In order to explain the peculiar advantage of large instruments, it is necessary to discard what may be looked upon as the fundamental principle of common optics, viz. the assumed infinitesimal character of the wave-length of light. It is probably for this reason that the subject of the present section is so little understood outside the circles of practical astronomers and mathematical physicists.

It is a simple consequence of Huyghens's principle, that the direction of a beam of limited width is to a certain extent indefinite. Consider the case of parallel light incident perpendicularly upon an infinite screen, in which is cut a circular aperture. According to the principle, the various points of the aperture may be regarded as secondary sources emitting synchronous vibrations. In the direction of original propagation the secondary vibrations are all in the same phase, and hence the intensity is as great as possible. In other direc-

* Communicated by the Author.

tions the intensity is less; but there will be no sensible discrepancy of phase, and therefore no sensible diminution of intensity, until the obliquity is such that the (greatest) projection of the diameter of the aperture upon the direction in question amounts to a sensible fraction of the wave-length of the light. So long as the extreme difference of phase is less than a quarter of a period, the resultant cannot differ much from the maximum; and thus there is little to choose between directions making with the principal direction less angles than that expressed in circular measure by dividing the quarter wave-length by the diameter of the aperture. Direct antagonism of phase commences when the projection amounts to half a wave-length. When the projection is twice as great, the phases range over a complete period, and it might be supposed at first sight that the secondary waves would neutralize one another. In consequence, however, of the preponderance of the middle parts of the aperture, complete neutralization does not occur until a higher obliquity is reached.

This indefiniteness of direction is sometimes said to be due to "diffraction" by the edge of the aperture—a mode of expression which I think misleading. From the point of view of the wave-theory, it is not the indefiniteness that requires explanation, but rather the smallness of its amount.

If the circular beam be received upon a perfect lens, an image is formed in the focal plane, in which *directions* are represented by *points*. The image accordingly consists of a central disk of light, surrounded by luminous rings of rapidly diminishing brightness. It was under this form that the problem was originally investigated by Airy*. The angular radius θ of the central disk is given by

$$\theta = 1.2197 \frac{\lambda}{2R}, \quad (1)$$

in which λ represents the wave-length of light, and $2R$ the (diameter of the) aperture.

In estimating theoretically the resolving-power of a telescope on a double star, we have to consider the illumination of the field due to the superposition of the two independent images. If the angular interval between the components of the star were equal to 2θ , the central disks would be just in contact. Under these conditions there can be no doubt that the star would appear to be fairly resolved, since the brightness of the external ring-systems is too small to produce any

* Camb. Phil. Trans. 1834.

material confusion, unless indeed the components are of very unequal magnitude.

The diminution of star-disks with increasing aperture was observed by W. Herschel; and in 1823 Fraunhofer formulated the law of inverse proportionality. In investigations extending over a long series of years, the advantage of a large aperture in separating the components of close double stars was fully examined by Dawes*. In a few instances it happened that a small companion was obscured by the first bright luminous ring in the image of a powerful neighbour. A diminution of aperture had then the effect of bringing the smaller star into a more favourable position for detection; but in general the advantage of increased aperture was very apparent even when attended by considerable aberration.

The resolving-power of telescopes was investigated also by Foucault†, who employed a scale of equal bright and dark alternate parts: it was found to be proportional to the aperture and independent of the focal length. In telescopes of the best construction the performance is not sensibly prejudiced by outstanding aberration, and the limit imposed by the finiteness of the waves of light is practically reached. Verdet‡ has compared Foucault's results with theory, and has drawn the conclusion that the radius of the visible part of the image of a luminous point was nearly equal to the half of the radius of the first dark ring.

Near the margin of the theoretical central disk the illumination is relatively very small, and consequently the observed diameter of a star-disk is sensibly less than that indicated in equation (1), how much less depending in some measure upon the brightness of the star. That bright stars give larger disks than faint stars is well known to practical observers.

With a high power, say 100 for each inch of aperture, the sharpness of an image given by a telescope is *necessarily* deteriorated, the apparent breadth of a point of light being at least $8\frac{1}{2}$ minutes. In this case the effective aperture of the eye is $\frac{1}{100}$ inch. In his paper on the limit of microscopic vision§, Helmholtz has shown that the aperture of the eye cannot be much contracted without impairing definition—from which it follows that the limit of the resolving-power of telescopes is attained with a very moderate magnification, probably about 20 for each inch in the aperture of the object-glass or mirror.

* Mem. Astron. Soc. vol. xxxv.

† *Ann. de l'Observ. de Paris*, t. v. 1858.

‡ *Leçons d'Optique Physique*, t. i. p. 309.

§ Pogg. *Ann.* Jubelband 1874.

We have seen that a certain width of beam is necessary to obtain a given resolving-power; but it does not follow that the whole of an available area of aperture ought to be used in order to get the best result. As the obliquity to the principal direction increases, the first antagonism of phase which sets in is between secondary waves issuing from marginal parts of the aperture; and thus the operation of the central parts is to retard the formation of the first dark ring. This unfavourable influence of the central rays upon resolving-power was well known to Herschel, who was in the habit of blocking them off by a cardboard stop. The image due to an annular aperture was calculated by Airy; and his results showed the contraction of the central disk and the augmented brightness of the surrounding rings*. More recently this subject has been ably treated by M. Ch. André†, who has especially considered the case in which the diameter of the central stop is half the full aperture. How far it would be advantageous to carry the operation of blocking out the central rays would doubtless depend upon the nature of the object under examination. Near the limit of the power of an instrument a variety of stops ought to be tried. Possibly the best rays to block out are those not quite at the centre (see § 2).

The fact that the action of the central rays may be disadvantageous shows that in the case of full aperture the best effect is not necessarily obtained when all the secondary waves arrive in the same phase at the focal point. If by a retardation of half a wave-length the phase of any particular ray is reversed, the result is of the same character as if that ray were stopped. Hence an exactly parabolic figure is not certainly the best for mirrors.

The character of the image of a luminous *line* cannot be immediately deduced from that of a luminous point. It has, however, been investigated by M. André, who finds that the first minimum of illumination occurs at a somewhat lower obliquity than in the case of a point. A double *line* is therefore probably more easily resolvable than a double *point*; but the difference is not great. In the case of a line the minima are not absolute zeros of illumination.

§ 2. *Rectangular Sections.*

The diffraction phenomena presented by beams of rectangular section are simpler in theory than when the section is circular; and they have a practical application in the spectro-

* See also *Astron. Month. Notices*, xxxiii. 1872.

† “*Étude de la Diffraction dans les Instruments d’Optique*,” *Ann. de l’École Norm.* v. 1876.

scope, when the beam is limited by prisms or gratings rather than by the object-glasses of the telescopes.

Supposing, for convenience, that the sides of the rectangle are horizontal and vertical, let the horizontal aperture be a and the vertical aperture be b . As in § 1, there will be no direct antagonism among the phases of the secondary waves issuing in an oblique horizontal direction, until the obliquity is such that the projection of the horizontal aperture a is equal to $\frac{1}{2}\lambda$. At an obliquity twice as great the phases range over a complete period; and, *since all parts of the horizontal aperture have an equal importance*, there is in this direction a complete absence of illumination. In like manner, a zero of illumination occurs in every horizontal direction upon which the projection of a amounts to an exact multiple of λ .

The complete solution of the present problem, applicable to all oblique directions, is given in Airy's 'Tracts,' 4th edition, p. 316, and in Verdet's *Leçons*, t. i. p. 265. If the focal length of the lens which receives the beam be f , the illumination I^2 at a point in the focal plane whose horizontal and vertical co-ordinates (measured from the focal point) are ξ, η , is given by

$$I^2 = \frac{a^2 b^2}{\lambda^2 f^2} \frac{\sin^2 \frac{\pi a \xi}{\lambda f}}{\frac{\pi^2 a^2 \xi^2}{\lambda^2 f^2}} \frac{\sin^2 \frac{\pi b \eta}{\lambda f}}{\frac{\pi^2 b^2 \eta^2}{\lambda^2 f^2}}, \quad . \quad . \quad . \quad (1)$$

the intensity of the incident light being unity. The image is traversed by *straight* vertical and horizontal lines of darkness, whose equations are respectively

$$\sin \frac{\pi a \xi}{\lambda f} = 0, \quad \sin \frac{\pi b \eta}{\lambda f} = 0. \quad . \quad . \quad . \quad (2)$$

The calculation of the image due to a luminous line (of uniform intensity) is facilitated in the present case by the fact that the law of distribution of brightness, as one coordinate varies, is independent of the value of the other coordinate. Thus the distribution of brightness in the image of a vertical line is given by

$$\int_{-\infty}^{+\infty} I^2 d\eta = \frac{a^2 b}{\lambda f} \frac{\sin^2 \frac{\pi a \xi}{\lambda f}}{\frac{\pi^2 a^2 \xi^2}{\lambda^2 f^2}}, \quad . \quad . \quad . \quad (3)$$

the same law as obtains for a luminous point when horizontal directions are alone considered. It follows from (3) that in the spectroscope *the definition is independent of the vertical aperture*.

In order to obtain a more precise idea of the character of the image of a luminous line, we must study the march of the function $u^{-2} \sin^2 u$. The roots occur when u is any multiple of π , except zero. The maximum value of the function is unity, and occurs when $u=0$. Other maxima of rapidly diminishing magnitude occur in positions not far removed from those lying midway between the roots. The image thus consists of a central band of half width corresponding to $u=\pi$, accompanied by lateral bands of width π , and of rapidly diminishing brightness. The accompanying Table and diagram (Plate VII. fig. 1) will give a sufficient idea of the distribution of brightness for our purpose.

TABLE I.

u .	$u^{-2} \sin^2 u$.	u .	$u^{-2} \sin^2 u$.
0	1.0000	π	.0000
$\frac{1}{6}\pi$	.9119	$\frac{5}{4}\pi$	.0324
$\frac{1}{4}\pi$	.8106	$\frac{3}{2}\pi$	.0427
$\frac{1}{3}\pi$	.6839	$\frac{7}{4}\pi$	.0450
$\frac{1}{2}\pi$	.4053	2π	.0165
$\frac{2}{3}\pi$	.1710	$\frac{5}{2}\pi$	.0000
$\frac{3}{4}\pi$	.0901	$\frac{3}{2}\pi$	.0162
$\frac{5}{8}\pi$	.0365	3π	.0000

The curve A B C D represents the values of $u^{-2} \sin^2 u$ from $u=0$ to $u=3\pi$. The part corresponding to negative values of u is similar, O A being a line of symmetry.

Let us now consider the distribution of brightness in the image of a double line whose components are of equal strength and at such an angular interval that the central line in the image of one coincides with the first zero of brightness in the image of the other. In fig. 1 the curve of brightness for one component is A B C D, and for the other O A' C'; and the curve representing half the combined brightnesses is E' B E F. The brightness (corresponding to B) midway between the two central points A, A' is .8106 of the brightness at the central points themselves. We may consider this to be about the limit of closeness at which there could be any decided appearance of resolution. The obliquity corresponding to $u=\pi$ is such that the phases of the secondary waves range over a complete period, *i. e.* such that the projection of the horizontal aperture upon this direction is one wave-length. We conclude that a double line cannot be fairly resolved unless its components subtend an angle exceeding that subtended by the wave-length of light at a distance equal to the horizontal aperture*.

* In the spectroscope the angular width of the slit should not exceed a moderate fraction of the angle defined in the text, if full resolving-power be wanted.

This rule is convenient on account of its simplicity; and it is sufficiently accurate in view of the necessary uncertainty as to what exactly is meant by resolution. Perhaps in practice somewhat more favourable conditions are necessary to secure a resolution that would be thought satisfactory.

If the angular interval between the components of the double line be half as great again as that supposed above, the brightness in the middle is $\cdot 1802$ ($2 \times \cdot 0901$) as against $1\cdot 0450$ ($1 + \cdot 0450$) at the central line. Such a falling off in the middle must be more than sufficient for resolution. If the angle subtended by the components of the double line be twice that subtended by the wave-length at a distance equal to the horizontal aperture, the central bands are just clear of one another, and there is a line of absolute blackness in the middle of the combined images.

On the supposition that a certain horizontal aperture is available, a question (similar to that considered in § 1) arises, as to whether the whole of it ought to be used in order to obtain the highest possible resolving-power. From fig. 1 we see that our object must be to depress the curve $ABCD$ at the point B . Now the phase of the resultant is that of the waves coming from the centre; and at the obliquity corresponding to B the phases of the secondary waves range over half a period. It is not difficult to see that the removal of some of the central waves will depress the intensity-curve at B , not only absolutely, but relatively to the depression produced at A . In order to illustrate this question, I have calculated the illumination in the various directions on the supposition that one sixth of the horizontal aperture is blocked off by a central screen. In this case the amplitude is represented by the function f , where

$$f = u^{-1} \left(\sin u - \sin \frac{u}{6} \right), \quad . \quad . \quad . \quad (4)$$

and, as usual, the intensity is represented by f^2 .

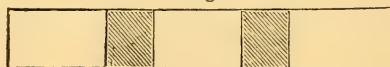
TABLE II.

u .	f .	$f^2 \div f_0^2$.	u .	f .	$f^2 \div f_0^2$.
0	+·8333	1·0000	$\frac{7}{4}\pi$.	−·2729	·1072
$\frac{1}{4}\pi$.	·7342	·7763	2π .	·1378	·0274
$\frac{1}{2}\pi$.	·4717	·3205	$\frac{9}{4}\pi$.	−·0307	·0014
$\frac{3}{4}\pi$.	+·1377	·0273	$\frac{12}{5}\pi$.	·0000	·0000
$\frac{6}{7}\pi$.	·0000	·0000	$\frac{5}{2}\pi$.	+·0043	·0000
π .	−·1592	·0365	$\frac{18}{7}\pi$.	·0000	·0000
$\frac{5}{4}\pi$.	·3351	·1617	$\frac{11}{4}\pi$.	−·0329	·0016
$\frac{3}{2}\pi$.	·3622	·1889	3π .	·1061	·0162

The third and sixth columns show the intensity in various directions relatively to the intensity in the principal direction ($u=0$); and the curve A B C D (fig. 2) exhibits the same results to the eye. A comparison with Table I. shows that a considerable advantage has been gained, the relative illumination at B being reduced from $\cdot4053$ to $\cdot3205$. On the other hand, the augmented brightness of the first lateral band (towards C) may be unfavourable to good definition. The second bright lateral band (towards D) is nearly obliterated. The curve E' B E F represents the resultant illumination due to a double line whose components are of the same strength, and at the same angular interval as before. The relatively much more decided drop at B indicates a considerable improvement in resolving-power, at least on a double line of this degree of closeness.

The increased importance of the first lateral band is a necessary consequence of the stoppage of the central rays; for in this direction the resultant has a phase *opposite* to that of the rays stopped. The defect may be avoided in great measure by blocking out rays somewhat removed from the centre on the two sides, and allowing the central rays themselves to pass. As an example, I have taken the case in which the two parts stopped have each a width of one eighth of the whole aperture, with centres situated at the points of trisection (fig. 3).

Fig. 3.



The function f suitable to this case is readily proved to be

$$f = u^{-1} \left(\sin u - 2 \sin \frac{u}{8} \cos \frac{u}{3} \right). \quad . \quad . \quad . \quad (5)$$

The values of f and $f^2 \div f_0^2$ are given in Table III.; and the intensity-curve A B C D is shown in fig. 4.

TABLE III.

u .	f .	$f^2 \div f_0^2$.	u .	f .	$f^2 \div f_0^2$.
0	+·75	1·0000	$\frac{3}{2}\pi$	-·2122	·0801
$\frac{1}{4}\pi$	·6594	·7727	$\frac{7}{4}\pi$	-·0689	·0084
$\frac{1}{2}\pi$	·4215	·3158	2π	+·1125	·0225
$\frac{3}{4}\pi$	+·1259	·0282	$\frac{5}{2}\pi$	·2189	·0852
π	-·1218	·0264	3π	·1960	·0684
$\frac{5}{4}\pi$	·2422	·1043			

The depression at B is even greater than in fig. 2, while the

rise at C is much less. Probably this arrangement is about as efficient as any.

I have endeavoured to test these conclusions experimentally with the spectroscope, using the double soda-line. The horizontal aperture of a single-prism instrument was narrowed by gradually advancing cardboard screens until there was scarcely any appearance of resolution. The interior rays were blocked out with vertical wires or needles, adjusted until they occupied the desired positions when seen through the telescope with eyepiece removed. With the arrangements either of fig. 2 or of fig. 4 a very decided improvement on the full aperture was observed; but there was no distinct difference between these two arrangements themselves. Indeed, no such difference was to be expected, since the brightness of the first lateral band has no bad effect on the combined images, as appears from the curve E' B E F (fig. 2). Under other circumstances the influence of the bright lateral band might be more unfavourable.

In powerful spectroscopes the beam is often rendered unsymmetrical in brightness by absorption. In such cases an improvement would probably be effected by stopping some of the rays on the preponderating side, for which purpose a sloping screen might be used giving a variable *vertical* aperture. It should be noticed, however, that it is only when the vertical aperture is constant that the image of a luminous line is immediately deducible from that of a luminous point.

§ 3. *Optical Power of Spectroscopes.*

As the power of a telescope is measured by the closeness of the double stars which it can resolve, so the power of a spectroscope ought to be measured by the closeness of the closest double lines in the spectrum which it is competent to resolve. In this sense it is possible for one instrument to be more powerful than a second in one part of the spectrum, while in another part of the spectrum the second instrument is more powerful than the first. The most striking cases of this inversion occur when one instrument is a diffraction-spectroscope and the other a dispersion-spectroscope. If the instruments are of equal power in the yellow region of the spectrum, the former will be the more powerful in the red, and the latter will be the more powerful in the green. In the present section I suppose the material and the workmanship to be perfect, and omit from consideration the effects of unsymmetrical absorption. Loss of light by reflection or by uniform absorption has no effect on resolving-power. Afterwards I propose to examine the effect of some of the errors most likely to occur in practice.

So far as relates to the diffraction-spectroscope, the problem

of the present section was solved in the *Philosophical Magazine* for March 1874. I there showed that if n denote the number of lines on a grating and m the order of the spectrum observed, a double line of wave-lengths λ and $\lambda + \delta\lambda$ will be just resolved (according to the standard of resolution defined in the previous section), provided

$$\frac{\delta\lambda}{\lambda} = \frac{1}{mn}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

which shows that the resolving-power varies directly as m and n . When the ruling is very close, m is always small (not exceeding 3 or 4); and even when a considerable number of spectra are formed, the use of an order higher than the third or fourth is often inconvenient in consequence of the overlapping. But if the difficulty of ruling a grating may be measured by the total number of lines (n), it would seem that the intervals ought not to be so small as to preclude the convenient use of at least the third and fourth spectra.

In the case of the soda- double line the difference of wave-lengths is a very little more than $\frac{1}{1000}$; so that, according to (1), about 1000 lines are necessary for resolution in the first spectrum. By experiment I found 1130*.

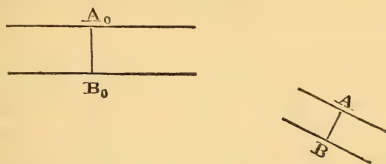
"Since a grating resolves in proportion to the total number of its grooves, it might be supposed that the defining-power depends on different principles in the case of gratings and prisms; but the distinction is not fundamental. The limit to definition arises in both cases from the impossibility of representing a line of light otherwise than by a band of finite though narrow width, the width in both cases depending on the horizontal aperture (for a given λ). If a grating and a prism have the same horizontal aperture and dispersion, they will have equal resolving-powers on the spectrum."

At the time the above paragraph was written, I was under the impression that the dispersion in a prismatic instrument depended on so many variable elements that no simple theory of its resolving-power was to be expected. Last autumn, while engaged upon some experiments with prisms, I was much struck with the inferiority of their spectra in comparison with those which I was in the habit of obtaining from gratings, and was led to calculate the resolving-power. I then found that the theory of the resolving-power of prisms is almost as simple as that of gratings.

* In my former paper this number is given as 1200. On reference to my notebook, I find that I then took the full width of the grating as an English inch. The 3000 lines cover a *Paris* inch, whence the above correction. From the nature of the case, however, the experiment does not admit of much accuracy.

Let $A_0 B_0$ (fig. 5) be a plane wave-surface of the light before it falls upon the prisms, $A B$ the corresponding wave-surface

Fig. 5.



for a particular part of the spectrum after the light has passed the prism or after it has passed the eyepiece of the observing-telescope. The path of a ray from the wave-surface $A_0 B_0$ to A or B is determined by the condition that the optical distance, represented by $\int \mu ds$, is a minimum; and as $A B$ is by supposition a wave-surface, this optical distance is the same for both points. Thus

$$\int \mu ds \text{ (for } A) = \int \mu ds \text{ (for } B). \quad . \quad . \quad . \quad (2)$$

We have now to consider the behaviour of light belonging to a neighbouring part of the spectrum. The path of a ray from the wave-surface $A_0 B_0$ to A is changed; but in virtue of the minimum property the change may be neglected in calculating the optical distance, as it influences the result by quantities of the second order only in the change of refrangibility. Accordingly the optical distance from $A_0 B_0$ to A is represented by $\int (\mu + \delta\mu) ds$, the integration being along the path $A_0 \dots A$; and, similarly, the optical distance between $A_0 B_0$ and B is represented by $\int (\mu + \delta\mu) ds$, where the integration is along the path $B_0 \dots B$. In virtue of (2) the *difference* of the optical distances is

$$\int \delta\mu ds \text{ (along } B_0 \dots B) - \int \delta\mu ds \text{ (along } A_0 \dots A). \quad . \quad (3)$$

The new wave-surface is formed in such a position that the optical distance is constant, and therefore the *dispersion*, or the angle through which the wave-surface is turned by the change in refrangibility, is found simply by dividing (3) by the distance $A B$. If, as in common flint-glass spectroscopes, there is only one dispersing substance, $\int \delta\mu ds = \delta\mu \cdot s$, where s is simply the thickness traversed by the ray. If we call the width of the emergent beam a , the dispersion is represented by $\delta\mu \frac{s_2 - s_1}{a}$, s_1 and s_2 being the thicknesses traversed by the extreme rays. In a properly constructed instrument s_1 is

negligible, and s_2 is the aggregate thickness of the prisms at their thick ends, which we will call t ; so that the dispersion (θ) is given by

$$\theta = \frac{t \delta \mu}{a}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

By § 2 the condition of resolution of a double line whose components subtend an angle θ is that θ must exceed $\lambda \div a$. Hence from (4), in order that a double line may be resolved whose components have indices μ and $\mu + \delta \mu$, it is necessary that t should exceed the value given by the following equation,

$$t = \frac{\lambda}{\delta \mu}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which expresses that the relative retardation of the extreme rays due to the change of refrangibility is the same (λ) as that incurred without a change of refrangibility when we pass from the principal direction to that corresponding to the first minimum of illumination.

That the resolving-power of a prismatic spectroscop of given dispersive material is proportional to the total thickness used, without regard to the number, angles, or setting of the prisms, is a most important, perhaps the most important, proposition in connexion with this subject. Hitherto in descriptions of spectroscopes far too much stress has been laid upon the amount of dispersion produced by the prisms; but this element by itself tells nothing as to the power of an instrument. It is well known that by a sufficiently close approach to a grazing emergence the dispersion of a prism of given thickness may be increased without limit; but there is no corresponding gain in resolving-power. So far as resolving-power is concerned, it is a matter of indifference whether dispersion be effected by the prisms or by the telescope. Two things only are necessary:—first, to use a thickness exceeding that prescribed by (5); secondly, to narrow the beam until it can be received by the pupil of the eye, or rather, since with full aperture the eye is not a perfect instrument, until its width is not more than one-third or one-fourth of the diameter of the pupil.

The value of expression (3) on which resolving-power depends is readily calculable in all cases of practical interest. For a compound prism of flint and crown, $\delta \mu \cdot t$ is replaced by

$$\delta \mu \cdot t - \delta \mu' \cdot t', \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where t and t' denote the respective thicknesses traversed, and $\delta\mu$, $\delta\mu'$ the corresponding variations of refractive index.

The relation between $\delta\mu$ and $\delta\lambda$ may generally be obtained with sufficient approximation from Cauchy's formula

$$\mu = A + B\lambda^{-2}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Thus

$$\delta\mu = -2B\lambda^{-3}\delta\lambda. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The value of B varies of course according to the material of the prisms. As an example I will take Chance's "extra-dense flint." The indices for C and the more refrangible D are*

$$\mu_D = 1.650388, \quad \mu_C = 1.644866;$$

so that

$$\mu_D - \mu_C = .005522. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Also

$$\lambda_D = 5.889 \times 10^{-5}, \quad \lambda_C = 6.562 \times 10^{-5},$$

the unit of length being the centimetre; whence by (7),

$$B = .984 \times 10^{-10}. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Thus by (5) and (8),

$$t = \frac{\lambda^4}{2B\delta\lambda} = \frac{10^{10}\lambda^4}{1.968\delta\lambda}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

For the soda-line,

$$\lambda = 5.889 \times 10^{-5}, \quad \delta\lambda = .006 \times 10^{-5};$$

and thus the thickness necessary to resolve this line is given by

$$t = 1.02 \text{ centimetres}. \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The number of times the power of a spectroscope exceeds that necessary to resolve the soda-lines might conveniently be taken as its practical measure. We learn from (12) that, according to this definition, the power of an instrument with simple prisms of "extra-dense glass" is expressed approximately by the number of centimetres of available thickness.

In order to confirm this theory, I have made some observations on the thickness necessary to resolve the soda-lines. The prism was of extra-dense glass of refractive index very nearly agreeing with that above specified, and had a refracting angle of 60° . Along one face sliding screens of cardboard were adapted, by which the horizontal aperture could be adjusted until, in the judgment of the observer, the line was

* Hopkinson, Proc. Roy. Soc. June 1877.

barely resolved. A soda-flame was generally used, though similar observations have been made upon the D line of the solar spectrum. When the adjustment was complete, the aperture along the face of the prism was measured, and gave at once the equivalent thickness, *i. e.* the *difference* of thicknesses traversed by the extreme rays, since the prism was in the position of minimum deviation. Care, of course, was taken that no ordinary optical imperfections of the apparatus interfered with the experiment.

One observer, familiar with astronomical work, fixed the point of resolution when the thickness amounted to from 1.00 to 1.20 centimetre. I was myself less easily satisfied, requiring from 1.35 to 1.40 centimetre. But even with a less thickness than 1 centimetre, it was evident that the object under examination was not a single line. With the same prism I found the thickness necessary to resolve $b_3 b_4$ in the solar spectrum to be about 2.5 centimetres. According to (11), the thickness required for $b_3 b_4$ should be 2.2 times that required for $D_1 D_2$. Probably something depends upon the relative intensities of the component lines.

From (1) and (11) we see that if a diffraction and a dispersion instrument have equal resolving-powers,

$$t = \frac{mn\lambda^3}{2B}; \quad . \quad . \quad . \quad . \quad . \quad (13)$$

so that the power of a dispersion instrument relatively to that of a diffraction instrument varies inversely as the third power of the wave-length.

For the kind of glass considered in (10), and for the region of the D lines,

$$t = 1.037 \frac{mn}{1000}. \quad . \quad . \quad . \quad . \quad (14)$$

To find what thickness is necessary to rival the fourth spectrum of a grating of 3000 lines, we have merely to put $m=4$, $n=3000$; so that the necessary thickness is about $12\frac{1}{2}$ centimetres—a result which abundantly explains the observations which led me to calculate the power of prisms.

Terling Place,
August 12, 1879.

[To be continued.]

XXXII. *Measuring Polariscopes.*

By Professor W. GRILLS ADAMS, M.A., F.R.S.*

[Plate VIII.]

SOME four years ago the description of a new measuring polariscope was communicated by the author to the Physical Society (see 'Proceedings,' vol. i. p. 152), in which the advantages gained are an extensive field of view combined with accurate means of measuring the rings and the separation of the optic axes in biaxial crystals. The peculiarity of the instrument consists in the arrangement of the two central lenses, one on each side of the crystal. These two lenses are plano-convex, very nearly hemispheres, and, with their flat surfaces inwards, form the two sides of a box to hold the crystal immersed in oil or a liquid; they are so placed that their convex surfaces form portions of the same spherical surface. The crystal is placed in the box at the centre of curvature of the spherical surfaces of the two lenses.

Two instruments have since been made on this principle with certain important modifications. In one, made by Mr. Tisley for horizontal projection (Plate VIII. fig. 1), the polarizer is a Nicol's prism capable of giving a clear parallel beam of polarized light $2\frac{1}{2}$ inches in diameter; the middle portion of the instrument (*i. e.* the box with the two equal central lenses for its two opposite sides) has an opening at the top, into which the crystal to be measured is inserted, and is adjusted to its right position by a cup-and-socket motion. When the angle between the optic axes is to be measured, the instrument is placed with its axis horizontal, the crystal is placed with the plane of the optic axis vertical, the box and crystal together are then turned about a horizontal axis at right angles to the direction of the axis of the instrument, *i. e.* at right angles to the plane of the optic axes: thus either of the optic axes of the crystal may be made to coincide with the centre of the field of view, where the spider-lines cross one another, the angle through which the box is turned being measured to minutes by means of a circle attached to it and a vernier attached to the fixed stand supporting the instrument.

A table-polariscope on the same principle has been made by Herr Schneider of Vienna, into which several important modifications have been introduced. A section and view of the instrument are shown in figs. 2 and 3.

The light falls on a plane mirror A, and is reflected into the

* Communicated by the Physical Society, having been read at the Meeting on June 28.

instrument (which is placed with its axis vertical) through the first lens B, which is fixed on the tube in which the polarizer C is placed.

D, E, F, G, and H (fig. 2) are lenses through which the light passes; and K is a Nicol's prism (the analyzer). This part of the instrument forms a complete table-polariscope of considerable range. The Nicol's prism C and the lenses are supported each by means of two screws (shown in fig. 3), which may be moved upwards or downwards in two slots, and the lenses fixed in their proper positions. Between the plano-convex lenses E and F are the two central lenses L, M, two portions of a sphere, between which the crystal to be measured is placed. The crystal should be immersed in oil. The setting of the lens L has a screw on its surface which fits into a screw in the setting of the other lens M, the arm supporting the lenses being a flat piece of metal one sixth of an inch thick, which is placed between them before they are screwed together. This arm, I, is supported by a stout crosspiece T, in the form of the arc of a circle which has its centre at the centre of curvature of the two lenses: the arc subtends nearly a right angle at the centre. It passes through and is supported by two guides P, Q, and has on its outer surface a rack which works with a small toothed wheel turned by the milled head N.

On the upper face of the arm I is a train of toothed wheels, the setting of the lens M being provided with teeth, so that, by means of the milled head S on the outer wheel, the lenses and crystal may be turned about the axis of the instrument.

The guides P and Q are attached to the face of a vertical circle which may be fastened by screws to a circular graduated rim R; this rim is supported on and turns upon another fixed vertical circle, which is provided with verniers, so that the angle through which the rotating circle is turned can be measured to one minute. The fixed circle is fastened to and carried by the piece of tubing holding the two lenses E and F, which is supported between the two pieces of tubing carrying the upper and lower lenses.

In this instrument the crystal can be turned about each of three axes which are mutually at right angles to one another.

I. By turning the vertical circle and tubing to which it is attached, the crystal is turned about the vertical axis of the instrument.

II. By turning the train of wheelwork the crystal can also be turned about an axis passing centrically through the central lenses, and the plane of its optic axes may be brought into the desired position for measurement.

III. By turning the arc T by means of the milled head

N, the axis of the central lenses may be made to coincide with the axis of the instrument; the plane of the optic axes is then parallel to the vertical fixed circle, the crystal being turned by this motion about an axis bisecting the obtuse angle between the optic axes.

IV. By turning the graduated circle with the arc T and the arm I attached to it on the face of the fixed vertical circle (*i. e.* round a horizontal axis perpendicular to the plane of the optic axes), and reading the verniers on both sides of the circle, the angles between the directions of the optic axes may be accurately measured.

XXXIII. *An Attempt at a Systematic Classification of the various Forms of Energy.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WHILE writing a little elementary manual of mechanics lately for Messrs. Chambers, my attention was directed to a certain amount of vagueness and loose language which appears to be current in modern statements concerning energy.

I venture, therefore, to ask you to publish in your Journal the following remarks on the subject, the greater part of which aim at embodying in a concise form what I understand to be the doctrines of the mathematical physicists (though they do not seem as yet to have been all clearly apprehended by physicists in general), while the remaining portion contains a few points of view which have not, as far as I am aware, been published; and though doubtless they have occurred to others besides myself, it would seem to be conducive to clearness of thought and accuracy of expression to have them briefly set forth in order, so that those which are erroneous or vague can be the better detected, and those which are true and definite can be the better apprehended. There can, I think, be little doubt that it may ultimately be possible, and that it is exceedingly desirable, to have all the fundamental doctrines of physics stated in ordinary language without technicalities; but unless such statements are accurate and devoid of vagueness, they can be of little or no use.

It will, I hope, be understood that the following attempts at statements are in a dogmatic form simply in order to be concise. Some improvement in the present language of textbooks is distinctly desirable; and I hope that this communication may be sufficiently suggestive to lead to a discussion,

or to an authoritative statement which shall conduce to a better understanding of the matter than is at present general.

I am, Gentlemen,

Your obedient Servant,

OLIVER J. LODGE.

1. Every action which takes place between two bodies* is of the nature of a *stress*. A stress consists of two equal opposite forces (called action and reaction, or force and anti-force), one of them exerted by the one body, and the other by the other; and it is impossible for one force to be exerted without the other.

2. Whenever a body exerting a force moves in the sense of the force it exerts, it is said to do *work*†; and whenever a body exerting a force moves in the sense opposite to that of the force it exerts, it is said to have work done upon it, or to do anti-work, the quantity of the work being measured in each case by the product of the force into the distance moved through in its own direction.

3. Whenever two bodies exert a stress on each other, they are in contact; and if they move, they move together over the same distance‡: hence, since the force equals the anti-force, the work done by the one in any movement is equal to the anti-work done by the other.

4. The working-power§ of a body is measured by the average force it can exert, multiplied by the *range* or distance through which it can exert it. The working-power of a body may be increased or diminished by increasing or diminishing either the force, or its range, or both; and it must remain dormant so long as external circumstances do not allow it to exert a force through a distance.

5. Whenever work is done *upon* a body, an effect is produced in it which is found to increase the working-power of

* The term *body* is here used in its most general sense, viz. as standing for a piece of matter in general, without regard to size. It may mean a planet or an atom; and it may even apply to such extra-material things as the æther and the hypothetical ultra-mundane corpuscles, or to any thing else which is sufficiently like ordinary matter to be capable of possessing energy and of doing work therewith.

† It seems preferable to speak of the work as being done by the *body* rather than by the *force*; though the latter expression is undoubtedly convenient sometimes.

‡ This step is rendered necessary by the preceding one of considering the work as done by the *body*. If it is the force which does the work, it is unnecessary.

§ Or power of doing work. But either term is objectionable, because *power* means *rate of doing work*. The term *entropy* has been used, but I believe that the accepted connotation of this word is now different.

that body (by an amount not greater than the work done); hence this effect is called *energy*, and it is measured by the quantity of work done in producing it*. Whenever work is done *by* a body, *i. e.* anti-work done on it, its working-power is found to be diminished (to at least the extent of the work done), and it is said to have lost energy—the energy lost being measured, as before, by the anti-work done in destroying it.

6. But in every action taking place between two bodies the work is equal to the antiwork (§ 3); hence the energy gained by the first body is equal to the energy lost by the second; or, on the whole, energy is neither produced nor destroyed, but is simply transferred from the second body to the first. (Remember footnote to § 1.)

To summarize then:—Work creates energy; anti-work destroys it; so both together simply transfer it. Or, in other words, the transference of energy requires a stress to act through a distance, and involves therefore two equal opposite works. If it were possible to obtain a force without its anti-force, or if it were possible for two bodies exerting stress on one another to move over unequal distances (§ 3), then it would be possible to obtain work without the anti-work, and thus to get a source of new energy (technically called the Perpetual Motion); but, as a fact of experience, it is not possible.

7. When work is done upon a body, different kinds of effects can be produced, depending both on the nature of the body and on the way in which the forces doing the work are

* This definition of energy, as the effect produced in a body by an act of work, is not so simple as the usual one—"the power of doing work;" but this latter definition seems a little unhappy. For energy is power of doing work in precisely the same sense as capital is the power of buying goods. Now a sovereign has an infinite power of buying goods if it has any at all—twenty-shillings worth being bought whenever it is transferred from one man to another. The proper statement is that a sovereign usually *confers upon the man that possesses it* a certain buying-power, which power he loses when he has transferred it; and in this sense money is a power of buying goods. It does not, however, *necessarily* confer upon its owner any buying-power, because there may not be any accessible person to buy from; and if there be, he may have nothing to sell. Just so with energy: it usually, though not necessarily (see § 14), confers upon the body possessing it a certain power of doing work, which power it loses when it has transferred it. The analogy here indicated will be found useful in teaching.

Energy corresponds to capital.

Doing work corresponds to buying.

Doing antiwork corresponds to selling.

The transfer of capital is accompanied by two equal opposite acts, buying and selling; and it is impossible for one to go on without the other. Hence the algebraic sum of all the buying in the world is always zero: this is the law of the conservation of capital.

applied to it; and these constitute the different *forms* of energy.

8. We can proceed to classify the forms of energy by first of all considering how the effects produced depend upon the forces applied to the body.

If these forces have no resultant (*i. e.* if they are in equilibrium), the body will be *strained*, and will exert a corresponding stress.

If the forces have a resultant, the body will be *moved**; and the motion will be either a translation or a rotation, or both, according as the forces can be reduced to a single finite resultant, a resultant zero at infinity, or to both combined.

Similarly, the strain may be analyzed into compression, elongation, and shear, or a combination of them, according to the way the forces act; but this division does not appear to be of much use for our present purpose.

All these effects are forms of energy, because the working-power of the body in which they are produced is in general increased; *i. e.* the body is rendered capable of doing work as soon as the proper condition is supplied. (See § 4.)

Thus a steadily strained elastic body is exerting force or pressure; but its point of application is stationary: allow it to move, and work is immediately done. A body in free motion is passing through space, but it is not exerting any force; supply a resistance, and work is immediately done.

9. Energy, therefore, has two principal forms:—

- (1) The free motion of bodies relatively to one another;
- (2) The separation of bodies from one another against stress.

And to these may be added for convenience the rapid alternation from one form to the other, called vibration.

10. The two fundamental forms of energy correspond to the two factors in the product work†. A body exerting force possesses energy, and a body moving through space possesses energy; but a body is not doing work unless it is *both* exerting force *and* moving through space.

* And possibly strained as well. It is only forces which like gravity act uniformly on every particle of a body that can move an ordinary elastic solid without straining it.

† Energy and work are not to be confounded together; and all such phrases as “accumulated work,” “conservation of work,” “conversion of heat into work,” “work consumed,” &c., should be eschewed. Energy is not work, but work can be got out of it if the proper condition be supplied. It is in fact *possible* work.

The expression *possible energy*, however, is meaningless; so also is the expression *actual energy*. All energy is *actual* and *real*—potential just as much as kinetic; and all represents *possible work*—that is, work that will become actual as soon as the other factor is supplied.

The energy possessed by matter in motion is called *Kinetic*. The energy possessed by matter exerting force is called *Potential*. It might with great propriety be called *Dynamic* energy; and it has been very conveniently called *Static* energy*, in opposition to kinetic. Of the two factors F and s , then, in the product work, kinetic energy corresponds to s ; there is motion through space, but no force: potential energy corresponds to F ; there is force, but no motion.

11. Whenever work is being done, both factors must be present—that is, both kinetic and potential energy; and the energy is always passing from one of these forms into the other while the work is being done. For if the motion of a body is *with* the force which acts upon it, its speed must increase; and if the motion is *against* the force, the speed must decrease; while in the first case the available distance through which the force can act, or the *range* of the force, is decreasing, in the second increasing.

12. The groups into which the forms of energy have been arranged (§ 8)—viz. strain, rotation, translation, and vibration—may now be subdivided further, by considering how the effects produced when work is done upon a body depend upon its nature and size.

A convenient division of bodies, according to size, will be—

1st. Masses comparable in size with the human body; which may be called ordinary masses.

2nd. Masses incomparably larger, as planets.

3rd. Masses incomparably smaller, as particles or molecules.

4th. The ultimate atoms.

All these material bodies agree in general properties, and differ only in size. But distinct apparently from these there exists an unknown *something*, which is material enough to be capable of possessing energy, to disturbances in which electrical phenomena seem to be due, and of which probably an aspect has been called æther. This must therefore constitute a 5th group, differing from the others apparently in respect of nature, not of size.

13. All these groups of bodies may be strained or set in motion in various ways when work is done upon them; and the groups into which the known forms of energy are thus thrown are exhibited provisionally in the following Table.

* The *cause* of the stress exerted by a strained body in any particular case is not in general known, and it may easily turn out often to be ultimately due to a kinetic phenomenon, as it certainly is in the case of the stress exerted by a compressed gas; nevertheless it may still be called static energy so long as the cause of the stress is not under consideration.

BODIES.	ENERGY OF MOTION, OR KINETIC ENERGY.		ENERGY ALTERNATELY KINETIC AND POTENTIAL.	ENERGY OF STRESS, OR POTENTIAL ENERGY.
	Rotation.	Translation.		
Planetary masses	1. <i>E. g.</i> Earth's diurnal motion.	2. <i>E. g.</i> Earth's annual motion.	3. <i>E. g.</i> The moon's libration. Tides. Pendulums.	4. Energy of gravitation. <i>E. g.</i> A bead of water. A raised weight.
	5. <i>E. g.</i> Fly-wheel.	6. <i>E. g.</i> Cannon-ball. Rivers.	7. Sound-vibrations. <i>E. g.</i> Tuning-fork.	8. Energy of strained elastic bodies. <i>E. g.</i> Watch-springs.
Particles or molecules ...	9. Part of the heat-energy of fluids.	10. Most of the heat-energy of gases.	11. Heat-energy of solids.	12. Energy of molecular stresses. <i>E. g.</i> "Internal work."
Atoms	13. Unknown motions which take place during the act of chemical combination and during dissociation.	14. The translation of atoms is observed in electrolysis.	15. The period of atomic vibration is observed by the spectroscope.	16. Energy of chemical affinity.
	17. Magnetism.	18. Electric currents.	19. (1) Discharge of accumulators. (2) Radiation.	20. (1) Electrostatic stress. (2) Electromagnetic stress.

[The numbers in the compartments are merely for convenience of reference.]

It is quite possible that the form of energy indicated in compartment No. 4 would be better placed in No. 8, those now in No. 8 being placed in No. 12; but I have placed them as they now stand because they are closely connected with the vibration-forms in the same rows. Moreover the true position of gravitation energy cannot be properly defined till we know more about it. It may have to come under the kinetic head—the motion of Le Sage's corpuscles.

Probably the arrangement of the forms in the last row may be improved, but I am not sufficiently acquainted with the Maxwellian theory to do it. Neither do I know whether one is justified in pointing out an analogy between the two forms of strain indicated in No. 20 and simple and torsional shear,—or whether one may imagine that the volume-elasticity and Young's modulus of the "something" are infinite, but that its rigidity is finite though high. An apparently consistent though rather hazy mental image of some obscure phenomena may be built up on a basis like this; but it is too speculative to be mentioned further here.

14. The power of doing work conferred upon a body by the possession of energy does not depend upon the absolute quantity of that energy only, but on its transferability. If it is not transferable, the body possessing it has no power of doing work.

15. Energy which can be guided, and all, or nearly all, transferred to any body at pleasure, is called a high or available form of energy, and is said to be capable of doing "useful" work, this work being done every time it is transferred in desired directions.

Energy which is nearly incapable of being guided, and which transfers itself in directions not required, is called a low or unavailable form of energy; and the work done at each of its undesired transfers is called "useless" work*.

16. The distinction between high and low forms of energy is a relative one, and depends on our present power of dealing with matter.

Masses of matter comparable to our own bodies in size can be handled and dealt with singly; and so they can in general be caused to do work upon, and therefore transfer their energy at pleasure to, any of the numerous accessible bodies which

* The distinction between useful and useless work is quite accidental, and belongs more to economics than to physics. An engineer will often degrade the whole of a large quantity of energy in order to produce some superficial result which he happens to desire at the moment, *e. g.* when a planing-machine smooths a surface; or when a locomotive transfers passengers or goods between places on the same level.

are competent to receive it. Hence energy possessed by them is generally of a high form.

17. Planetary masses can be dealt with singly indeed, but so singly that there is scarcely* any other body accessible to which their motion can be transferred† (see §§ 4 and 14).

18. The energy of moving molecules is not very available to us, because we can only deal with them statistically and not individually. There is a large amount of relative motion and transference of energy constantly going on among individual molecules; but, as we have no control over it, the work done is useless, and the energy unavailable. The only part of the energy which can be transferred at will to external bodies is that due to the *average* state of the moving molecules; and it is not possible to transfer even this unless some other mass is accessible, the average state of whose molecules in respect of motion or strain is in some way different, so that the one is able to do work upon the other‡.

Now since all accessible bodies have very large stores of molecular energy, it follows that a very great portion of the energy which belongs to the molecules of a body must be totally unavailable to us, because it can never be got rid of or transferred. And even the portion which can be transferred at pleasure to some larger body, if not made use of quickly, will be found to transfer itself to neighbouring molecules and in directions not required, and will waste itself in doing useless work. Hence molecular energy is called a low form.

19. Atomic or chemical energy seems at present to rank a little higher than molecular energy; for though one way of availing ourselves of it is by converting it into molecular energy (heat) and then doing useful work with the balance of the average effect by which the body heated excels its neighbours, yet animals and galvanic batteries are able to do useful work with it in a more direct and less wasteful fashion.

*The well-known exception is the ocean, which by the agency of the moon is put into a slightly different state of motion from the rest of the earth; and a minute portion of the earth's energy of rotation is constantly being transferred to it. A portion of this tidal energy is now available to us, and may be made to do useful work.

† Hence the kinetic energy of the earth is of no more use to us than a bank-note to Robinson Crusoe.

‡ An analogy may be drawn between the molecular energy of a body and the money of a bank; of which a reserve fund is kept for internal transfer and transactions between customers, while the excess gets invested in external concerns which have a deficiency, and so becomes available for doing useful work. To make the analogy more complete, the clerks should be uniformly dishonest, or the coffers insecure, so that stored money should dribble away.

The unknown, or electrical, energy appears to rank distinctly above the energy of molecules; because we have found some remarkable and indirect means of transferring the energy of electric currents to ordinary masses, by the intervention of electromagnetism, with a comparatively small waste.

20. When energy passes from a higher to a lower form it is said to be *degraded*; and when it has no availability at all it is called *dissipated**.

Energy is degraded when it is transferred from masses of ordinary size to the molecules of which they or others consist (§ 18).

The two fundamental forms of energy are those due to motion and those due to strain (§ 9). Now whenever motion takes place against friction, some energy is always transferred to the molecules of the rubbing surfaces. And whenever strain is produced in imperfectly elastic bodies, some energy always passes to the molecules.

But in practice no motion takes place without friction, and all bodies are imperfectly elastic. Hence energy is continually getting dissipated; or, in other words, at every transfer of energy between ordinary bodies under ordinary circumstances, some of it is always and necessarily degraded into a lower and less available form†.

It may be useful to append the following summary of the contents of the sections:—

1. Newton's third law.
2. Definition of work, + and —.
3. Denial of "action at a distance."
4. Definition of working-power.
5. Definition of energy.
6. Conservation of energy, and first law of thermodynamics.
7. Possibility of various forms of energy.
8. Classification of the forms of energy.
9. The fundamental forms of energy.
10. Kinetic and potential energy are related to the two factors in the product work.

* It is then of no more use to us than is our money to the inhabitants of Mars, who have no means of getting at it. Its terrestrial transferences are to them useless.

† For instance, during every quarter-swing of a free pendulum, energy is being transformed from kinetic to potential, or *vice versa*; and is being transferred from the unknown gravitation agent to the mass of the pendulum, or back again. Some, however, is dissipated every time, and ultimately the pendulum must stop.

11. Transformation from one form to the other.
 12. Further subdivision of the forms of energy.
 13. Classification table.
-
14. Distinction between energy and what was once called entropy.
 15. Distinction between available and unavailable energy ; and between useful and useless work.
 16. Reason why the energy of ordinary masses is available.
 17. Reason why planetary energy is almost unavailable.
 18. Reasons why molecular energy is much of it unavailable ; and second law of thermodynamics.
 19. Extent of availability of atomic and of electrical energy.
 20. Dissipation of energy.
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XXXIV. *A Mode of producing Arago's Rotation.*

By WALTER BAILY, M.A.*

[Plates IX. & X.]

A RAGO'S method of producing rotation in a copper disk consists of suspending it by its centre so as to make it lie horizontally above the poles of a horseshoe magnet, and then rotating the magnet about a vertical axis. The rotation of the disk is due to that of the magnetic field in which it is suspended; and we should expect that if a similar motion of the field could be produced by any other means, the result would be a similar motion of the disk.

Possibly the rotation of the magnet may be the only practicable way of producing a uniform rotation of the field ; but it will be shown in this paper that the disk can be made to rotate by an intermittent rotation of the field effected by means of electromagnets.

Suppose two magnetic poles to be below a plane sheet of a conducting substance capable of moving in its own plane. Each pole may be regarded as a small circular current parallel to the disk. The currents will be in the same or different directions according as the poles are of the same or different names. We will examine the effect of a change in the strength of either of the poles, in giving the sheet a tendency to move.

There are four cases ; viz.—

1. Poles alike. One increasing.
2. „ „ One diminishing.
3. Poles unlike. One increasing.
4. „ „ One diminishing.

* Communicated by the Physical Society, having been read at the Meeting on June 28.

In case (1) the increasing pole induces in the portion of the sheet opposite to itself a circular current opposite in direction to the currents representing the poles. Hence this portion of the sheet is repelled by both poles. The repulsion from the increasing pole is perpendicular to the sheet, and gives it no tendency to move; but the repulsion from the other pole tends to move the sheet from the constant pole toward the one which is increasing.

In case (2) the diminishing pole induces in the portion of the sheet opposite to it a circular current in the same direction as those representing the poles; so that this portion of the disk is attracted by both poles, and the disk tends to move from the diminishing pole toward the one which is constant.

In cases (3) and (4) it can easily be shown that the results are the opposite to those obtained in cases (1) and (2) respectively.

If one pole increases while the other diminishes, both tendencies to move are in the same direction, and the resulting tendency is the sum of the two.

The pole of an electromagnet made or broken is the extreme case of a pole increasing or diminishing.

Now conceive an even number of vertical bar electromagnets arranged in a circle with their upper poles in one horizontal plane, and a copper disk to be suspended above them; and the two magnets at the extremities of each diameter to be coupled together, and with a battery, so that each such pair of magnets forms a horseshoe electromagnet independent of all the others. Let P, Q, R, S be pairs of magnets at the ends of successive diameters. Make P, and then make Q so that the north pole of Q is adjacent to the north pole of P, and therefore the south pole of Q adjacent to the south pole of P. Then by case (1) the portion of the disk opposite the north pole of P is driven towards the north pole of Q; and a similar action takes place at the south poles. Now break P. By case (2) the portion opposite the north pole of P is again driven towards the north pole of Q, and so with the south poles. Continuing the action by making R and then breaking Q, making S and then breaking R, and so on, in each case making the adjacent poles similar, we get a series of impulses on the disk all tending to make it move in one direction round the axis of suspension. Hence the disk will rotate as in Arago's experiment.

In one extreme case, viz. when the number of electromagnets is infinite, we have the case of a uniform rotation of the magnetic field, such as we obtain by rotating permanent magnets.

In the other extreme case the number of pairs of electromagnets is reduced to two, and the number of batteries is also reduced to two. Let the poles of one pair of magnets be called a, a' , and those of the other b, b' . Then the arrangement of the pole as seen from above is $\begin{smallmatrix} a & b \\ b' & a' \end{smallmatrix}$; and the successive states of these poles will be

1.	2.	3.	4.	5.	6.	7.	8.	1.
NN	ON	SN	SO	SS	OS	NS	NO	NN
SS	SO	SN	ON	NN	NO	NS	OS	SS

N, S, representing north and south poles respectively, and O representing that the pole is not magnetic. It appears that the change from 1 to 3 through 2 is nothing more than reversing $a a'$; and the change from 3 to 5 consists in reversing $b b'$. Similarly we pass from 5 to 7 by reversing $a a'$, and again from 7 to 1 by reversing $b b'$. The whole process is thus shown to consist in reversing $a a'$ and $b b'$ alternately.

In passing from 8 to 1 we see by case (1) that the parts of the disk over b, b' are respectively repelled from a, a' ; and by case (3) we see that the same parts are respectively attracted to a', a . Again in passing from 1 to 2 we see by case (2) that the parts above a, a' are respectively attracted by b, b' ; and we also see by case (4) that the same parts are respectively repelled by $b' b$.

The effect of each of these two changes is to make the disk tend to rotate in the direction $a b a' b'$. All the other changes may be shown to have the same effect; so that the disk will rotate in the direction $a b a' b'$.

If starting with the state (1) we reverse $b b'$ first, we should have the series of states as follows:—

1.	2.	3.	4.	5.	6.	7.	8.	1.
NN	NO	NS	OS	SS	SO	SN	ON	NN
SS	OS	NS	NO	NN	ON	SN	SO	SS

from which it may be easily shown that the disk would rotate in the direction $b a b' a'$.

The experiment with the four electromagnets may be readily performed by means of a commutator which will reverse the currents several times in a second; and a considerable rotation can be given to the disk.

The commutator which I constructed for the experiment consisted of a wheel of wood with a brass rim. This rim was completely cut through in places equidistant from each other; and ten tongues of thin copper pressed against the rim*.

* The number of cuts must not be less than seven. I used eight cuts.

These tongues were in two groups of five ; and in each group the distances between the contacts of the tongues with the rim were half the distances between the cuts in the rim. The distance between the two groups must be greater than the distance between the contacts.

Let A be a wire from the positive pole of our battery, and $A' A''$ be wires from the negative pole of the same battery; B a wire from the positive pole of the other battery, and $B' B''$ wires from the negative pole ; and let aa' be the ends of the coil round one pair of electromagnets, and bb' the ends of the coil round the other pair. In the figures 1. to 4 the wheel is seen in four consecutive positions, with the tongues in contact with it ; and the letters show with what wire each tongue is connected.

The contact of two tongues with the same section of the ring puts the wires to them into electric connexion.

The connexions are, in

- | | | | | |
|---------|---------|----------|---------|----------|
| Fig. 1. | Aa , | $A'a'$, | Bb , | $B'b'$, |
| Fig. 2. | Aa' , | $A''a$, | Bb , | $B'u'$, |
| Fig. 3. | Aa' , | $A''a$, | Bb' , | $B''b$ |
| Fig. 4. | Aa , | $A'a'$, | Bb' , | $B''b$ |
| Fig. 1. | Aa , | $A'a'$, | Bb , | $B'b'$. |

Hence in passing from fig. 1 to fig. 2 the current through aa' is reversed ; in passing to fig. 3 that through bb' is reversed. The current through aa' is reversed again in passing to fig. 4, and that through bb' is reversed again in passing to fig. 1. The commutator is thus seen to reverse each pair of magnets twice while rotating through the angle subtended by one division of the rim ; so that with eight divisions one turn of the wheel reverses each pair of magnets sixteen times.

If the wheel is rotated in the opposite direction, the series of magnetic states is obtained in the reverse order, and the disk rotates in the opposite direction. But there is a better method of reversing the motion of the disk—which is, to introduce an ordinary commutator into one of the circuits, either between the battery and the wheel, or between the wheel and the magnets. The reversal of this commutator reverses the motion, while the wheel is rotated continuously in one direction. Fig. 5 gives the arrangement of the connexions. A, B are the batteries ; a, a', b, b' the electromagnets ; C the ordinary commutator ; and D the wheel.

It should be noted that the rotation of the disk is accompanied by the formation of induced currents whose intensity depends on the velocity of rotation, and whose effect is to diminish the rotation. These opposing currents are got rid of

when the disk is suspended as a torsion-balance, and its deflection observed, as the currents will not be formed except when the disk is moving.

The effect on the disk might be much increased by placing four other electromagnets above the disk, each opposite one of the lower magnets, as connected with it, so that the lower pole of the upper magnet should be of the opposite name to the upper pole of the lower magnet. In fig. 6 one pair of magnets is shown with the opposite pair, and the wires connecting them. The disk is seen in section, balanced on a needle-point, between the two pairs of magnets. The other four magnets are not shown in the figure.

XXXV. *On the Present State of Experimental Acoustics, with Suggestions for the Arrangement of an Acoustic Laboratory, and a Sketch of Research.* By R. H. M. BOSANQUET, St. John's College, Cambridge.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following paper presents an outline of suggested arrangements and work for an acoustic laboratory, which I hope shortly to be able to carry out. I have thought that it may be of interest to the readers of the Philosophical Magazine.

Yours truly,

R. H. M. BOSANQUET.

Experimental acoustics are at present in a condition which is perhaps not entirely satisfactory. In the teaching of the subject there is occasionally more demand upon the faith of the learner than is altogether desirable in an experimental science. I think that this arises from the difficulty of access to those experiments which deal with the foundations of the science. The prices charged for complete sets of acoustic apparatus are enough to show that the possession of such apparatus must be confined to few. Adequate sets of such apparatus, used in a sufficient and convincing manner, are exceedingly rare. Under these circumstances, that full experimental knowledge which is desirable in a science of this description does not generally exist.

The ordinary apparatus and arrangements for demonstration appear to err in some points. The effects are not produced in a continuous manner, but by fits and starts, generally by bowing on the sounding body. We do not analyze with ease

and certainly a phenomenon which only presents itself to disappear again. These intermittent phenomena are generally produced by an effort, often requiring considerable skill. Under these circumstances there is a tendency to accept the first conclusion that comes to hand, the mind being to some extent satisfied with the production of the difficult phenomenon. Again it has become perhaps too much the practice to refer the phenomena to optical analysis or analogy. In some cases this reference is, no doubt, most convenient; in other cases it is misleading. It is requisite that the analysis of the perceptions of the ear be conducted by reference to the ear itself.

The only form of apparatus in use for the production of simple tones in a continuous manner depends on the application to resonators of tuning-forks driven by electromagnets. These are in many respects ill adapted for demonstration, though no doubt they have furnished most valuable results. There appears to be a want of adaptability about the apparatus; and it is very costly. Professor König is probably better acquainted with the practical use of this apparatus than any one else, except perhaps Helmholtz himself; and König has, in an elaborate analysis of the phenomena*, controverted the entire foundations of the work of Helmholtz. Opinions are by no means at one on the subject, even amongst the highest authorities.

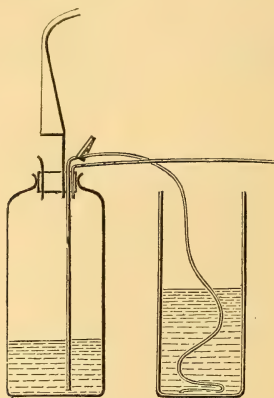
With the object of improving the treatment of this part of the subject, I have introduced resonators which speak in the manner of organ-pipes. These resonators are easily managed with a little simple organ-mechanism; they receive their wind through flexible tubes, and can be placed in any part of the room, at any distance apart, &c.—a matter of great importance in facilitating the analysis of the phenomena. The resonators are fitted with siphons and reservoirs; by a simple contrivance of this kind they can be tuned to any note within range at a moment's notice.

These resonators are made out of bottles, corks, and metal organ-pipes. Their cost is trifling.

The notes should be perfectly pure tones, according to the theory, having regard only to disturbances of the smallest order: but practically, in all tones of this supposed pure character small quantities of harmonics do exist; and I have long maintained that they always must exist in sensible intensity, on account of the transforming power of the air, or, in other

* Pogg. *Ann.* clvii. p. 177; *Phil. Mag.* [V.] i. pp. 417, 511. See also 'Proceedings of Musical Association,' 1878-79, Spottiswoode "On Beats and Combination-tones."

words, because the aerial disturbances of higher orders are never small enough to be entirely neglected*. The lower harmonies can be accordingly detected in the notes of these resonators by the use of analyzing resonators of proper pitch, whose interiors communicate with the ear. The twelfth can be detected by an experienced ear, in some cases, without the use of analyzing resonators.



Section of speaking resonator, with organ-pipe mouthpiece, siphon with stop, and reservoir for tuning; also flexible tube for putting into the ear when used as an analyzing resonator. This tube can be also used for gas-flame experiments.

With a rough experimental bellows furnishing an unsteady wind, and these resonators, I have been able to repeat some of the less difficult of the experiments of König.

The only statement made by König as to the notes of which the beats consist, is in *Phil. Mag.* fifth series, i. p. 425, where he says that the two notes of a harmonic interval appear alternately, but that the observation is difficult in the case of the octave. Now in all cases where beats exist, it is possible, by the use of analyzing resonators communicating with the ear, to determine in the manner pointed out by Helmholtz the different notes which vary in intensity, as well as any that do not vary in intensity. But König does not appear to have attempted this analysis at all.

In the cases I have examined I have succeeded in determining the notes which were beating. In this determination, which is sometimes of difficulty, I found it useful not only to employ the analyzing resonators, but to move them about, as

* See abstract of a paper "On the Conditions of the Transformation of Pendulum-Vibrations," Report of British Association, 1876 (*Transactions of Sections*, p. 45).

well as the primary notes, until the effects were most distinctly obtained. Stationary nodes and loops are formed in the room for all the notes present; and by taking advantage of these the analysis may be much assisted.

Curious results are undoubtedly obtained. For instance, the beats of an octave slightly out of tune are almost entirely on the lower note. If the notes are kept apart in the room, the upper one appears quite steady; while the lower one varies much in intensity, whether we listen to it with the unassisted ear or with a resonator. With the assistance of a resonator the much smaller variation of the upper note can be detected. These two phenomena are not separated by König, but described simply as a beat. According to Helmholtz, the beat of the lower note would be due to the difference-tone, that of the upper note to the octave harmonic of the lower note interfering with the upper one. Now it is easy to convince oneself with the analyzing resonator that the latter interpretation of the beat of the upper note is right: one can hear separately the octave harmonic and the octave note, as well as the beat itself. But the explanation of the beat of the lower note, as due to the interference of the difference-tone, presents this difficulty. If we run up the lower note by means of the siphon, the difference-tone should become audible. But with the arrangements I employed no trace of difference-tone could be perceived, even when the octave was run up to a fifth and a sensitive resonator employed to detect the difference-tone. The notes were placed far apart; and the arrangement was, no doubt, not favourable to the production of a difference-tone: but then how could it be there, so as to form these strong beats of the lower note, if it was not loud enough to be heard separately? I have only stated the above with the object of showing that there is a large ground here, important in a theoretical point of view, which will repay careful working. In fact the repetition and examination of the large collections of experiments in the work of König above cited, with the notes of such resonators as I have described, or with pure notes equally powerful and manageable, is a work essential to the establishment of the foundations of the theory of the subject.

I have come to the conclusion that it is not practicable to carry the work much further with this apparatus without a much steadier wind. For this purpose it is desirable to construct improved bellows. No existing pattern (except that of Cavaillé Coll) delivers a wind of any thing like the steadiness desirable. I shall return to this question later.

Although with an improved bellows hand blowing would be admissible, yet it is always difficult to secure uniformity in

hand-blown wind; and where these investigations are to be made on any large scale it will be preferable to employ a small engine, of a kind affording uniform rates of motion, for this as well as for many other acoustic experiments.

This suggestion forms the key to a conception of an acoustic laboratory, which is, I believe, new, and would, it seems to me, constitute a considerable step from an experimental point of view. This is, that, in an acoustic laboratory, power should be employed to produce all the effects required, in a continuous manner where continuity is suitable, and without effort or attention on the part of the investigator when the experiment is once arranged. It is easily seen that determinations of all sorts, which now present almost insuperable difficulties, would become perfectly easy under such conditions; and accurate knowledge would soon take the place of many of our guesses of today, through numberless extensions of the work now possible.

I proceed to mention the principal other subjects which suggest themselves as suitable for research under these conditions.

Aerial Mechanics generally.

So far as our knowledge of the mechanics of fluids, and especially of the air, has progressed beyond its elements of late years, the progress has been mainly in the mathematics of the subject. No doubt the deficiency of experiments arises mainly from the difficult nature of the subject matter; but it is also certainly due to the small extent to which real effort has been made to devise experiments of easy and certain execution, which shall supplement and check the mathematical investigations. No doubt a beginning has been made, and the experiments which are known have been made to tell their tale with admirable perseverance and ingenuity; but it is my opinion that the mathematical structure has, in some respects, been built up too rapidly.

The questions which arise are of great difficulty; and I do not purpose here to enter on any of them. It is sufficient to say that I believe there will be no difficulty in devising a number of experiments by which the simpler cases of aerial motion may be examined in detail. Until the whole circumstances of crucial experiments of this kind have been made out experimentally, I shall continue to feel grave doubts as to the stability of certain portions of the mathematical edifice.

I may allude to a few experiments of importance. Much has been built on a mathematical solution of the following problem:—If a circular disk, closing a circular hole in an infinite plane, execute oscillations at right angles to the plane,

what is the motion thereby imparted to the surrounding air? There is a question how far the solutions of this problem, which have been given, conform to the facts. The experimental settlement of this question is quite possible; and it goes to the very foundation of a certain portion of the mathematical equations upon which the modern theory is built.

Again, the equivalence of sound with mechanical energy is at present in the position of a mathematical speculation. The establishment of this equivalence, with a systematic mode of measurement, and the determination of the various laws on which it depends, is a work which alone would constitute an important research. I shall return to this later.

There is another important question in aerial mechanics, which calls for mention among the very first. The whole theory as now developed, neglects intentionally the viscosity of the air. Now the effects of viscosity are more easily dealt with by experiment than by theory. It is pretty certain that the effects of viscosity are not really negligible; for if they were, vortex rings could not be produced in air, nor could they be extinguished when once produced; and in fact, as we all know, they can be produced, and are rapidly extinguished.

The actual composition of the notes produced by the various methods which aim at the production of simple tones is also a matter of primary importance, which no attempt is being made to settle. It is certain that the existing explanation of even such a simple matter as the overtones of organ-pipes is insufficient; for speaking resonators, whose overtones should by theory be inharmonious, give true harmonics, of small intensity, indeed, but with a general effect not very dissimilar to that heretofore supposed to be peculiar to stopped organ-pipes. It is possible that the organ-pipe mouth is responsible for much more than has been supposed.

I have noticed these points only as specimens of aerial mechanics. The oscillating disk first mentioned will be easily constructed, and serve for numerous experiments of this class. On the whole, experimental aerial mechanics must be regarded as, in the future, probably the most important part of experimental acoustics.

Vibration Numbers.

The accurate determination of the vibration numbers or frequencies of notes is at present a matter of great difficulty. Where many such determinations are to be made, the employment of a small engine possessing a uniform rate of speed, controlled by a contrivance to be described later, will place this part of the subject on a new footing. The siren, the revolving stopcock (described later and already constructed),

the old toothed-wheel apparatus, and the flashing machine of Lord Rayleigh are examples of instruments which will acquire a new importance for this purpose when driven by a uniform motor.

Revolving Stopcock.

There is a large class of investigations which depend on the regular opening and closing of a channel, for the interruption either of a current of wind or of a current of sound. A turntable fitted with revolving stopcocks has been constructed for these investigations; and rough results are obtained without difficulty; but it is only by the employment of the uniform motor that accurate results can be expected.

The dissipation of sound in resonators and organ-pipes is a problem which may be attacked by means of the interrupted current of wind. What is the length of a periodic interruption in the wind-supply of a pipe or resonator which just fails to break the continuity of the tone? The answer to this question, and the phenomena we come across in the process of obtaining it, furnish important contributions towards our knowledge of aerial mechanics.

Another problem depending on interrupted wind-supply is, the determination of the velocity of sound in the open air for different musical notes, by a process bearing some slight analogy to Fizeau's determination of the velocity of light by reflection from a distance. The corridor of a cloister supplies an excellent *locale*. The sound is emitted by the apparatus at such intervals that a number of echoes (4, 6, or 8) are heard between two successive sounds. The mode of calculation is obvious.

A problem depending on the interrupted current of sound is, If the ear listen to a sound through the interrupted channel, what are the phenomena presented? They are of some complexity and considerable interest. Professor Mayer has published experiments on the subject.

Reeds.

Notwithstanding the investigations that exist on the behaviour of reeds with respect to columns of air with which they are connected, the subject is still involved in considerable obscurity. There are different kinds of reeds, which possess very different properties; and there is ample room for a thorough experimental investigation. With the appliances of the proposed laboratory these investigations are within reach.

The revolving stopcock can be used to admit wind to the bottom of a pipe or resonator, and so separate out those effects which may be regarded as the results of inexorable motions of

reeds from those in which the reeds are influenced by the reaction of the resonator. This arrangement gives rise to a beautiful set of experiments having many bearings. The trouble of maintaining the constant motion of the turn-table is very great; and it is practically impossible to obtain definite results without the uniform motor.

This arrangement, where the stopcock delivers wind into a pipe or resonator having the same vibration-frequency as that of the jet of the stopcock, gives a smooth powerful tone: it is well fitted for the evaluation of a sound of given loudness in terms of mechanical energy. I shall return to this point.

Strings.

The conditions of the flow of sound-energy from strings, through sound-boards, into the surrounding air require investigation. The case of practical interest is that of the violin. By arranging a sort of skeleton so as to represent the principal parts of the instrument, and employing mechanical bowing, it is expected that some light may be thrown on this obscure subject. This question is as yet untouched; but it is probable that the bridge and sound-post transmit a longitudinal vibration, which is communicated to the back at the point where it meets the sound-post at right angles. The effect of "muting," or loading the bridge with a small weight, comes in as a question for explanation.

Orchestral Instruments.

The study of the theory of orchestral instruments is in its infancy. The theory of the fingering of the wood wind—flute, hautboy (oboe), clarionet, bassoon—appears likely to be for the most part tolerably straight forward. The cases where two or more segments of a tube affect each other, though there are open holes between, form a problem which is untouched.

The law that in all lip reed-instruments the note produced is a resonance-note of the tube, was enunciated and proved first by Mr. Blaikley (see 'Proceedings of the Musical Association,' 1877-78, p. 56). On the same occasion I stated that I had obtained and proved the same law experimentally for the hautboy and clarionet (*l. c.* p. 62). We may therefore take as the basis of our work the proposition that, when reeds of movable pitch form notes in combination with a variable resonance, the note produced coincides with a note of resonance. This is not true for reeds of fixed pitch associated with a resonance, as in organ reed-pipes, according to the best practical authorities.

The study of the partial tones of columns of air, such as are

enclosed in ordinary brass instruments, is of high practical importance. This study has lately for the first time been put on a sound footing by Mr. Blaikley (*l. c.*). The examination of different forms opens up a considerable field of work.

Changes of Temperature.

The effect of changes of temperature on sounding columns of air, tuning-forks, and other sounding bodies, still requires investigation. It remains unexplained, for instance, why small organ-pipes are more affected by changes of temperature than large ones. The accurate laws of the change remain also to be ascertained.

Velocity of Sound in Tubes.

The laws of the variation of the velocity of musical sounds of different pitch, in tubes of varying diameter, have been formulated*; but the results obtained by different investigators do not agree, and this important element is consequently uncertain to quite a considerable extent. There appears to be no reason why this should not be cleared up by the use of proper appliances.

Quality of Organ-pipes.

The mechanical conditions under which sound of different qualities is produced are not understood in all cases. We know empirically that an organ-pipe of large diameter gives a pure tone. In fact the largest-scaled open organ-pipes have their fundamentals so predominant that analysis by beats fails to detect any harmonics. For investigations as to the lowest limit of audible sounds there is, therefore, no apparatus to be compared with a large-scale 32-foot open diapason as it stands on the organ. The notion that stopped pipes are preferable is a mistake. Whether it be that stopped pipes are not made of sufficiently large scale I do not know; but it is generally easy to demonstrate, by a simple process of analysis by beats, that stopped pipes drop their fundamental about the middle of the 32-foot octave, or at about 25 vibrations per second, whereas with open 32's the fundamental remains approximately unmixed to the very lowest pair of notes. As we diminish the depth of the pipe from back to front, the predominance of the fundamental diminishes; and as we continue, we come to a point where the pipe cannot be made to speak its fundamental at all. Further investigation is required.

* See Pogg. *Ann.* new series, ii. p. 235; also Pogg. *Ann.* cxxxiv. p. 177.

Sympathy and Drawing.

Under certain circumstances two sources of sound react on each other, and affect either the pitch or intensity with which they would speak separately. When two organ-pipes of the same pitch weaken each other's intensity, there is said to be "sympathy." When two of different pitch affect the pitch in which they mutually speak, there is said to be "drawing." Lord Rayleigh brought forward some cases of the latter (see Proc. Musical Assoc. 1878-79, p. 26), in which the pipes spoke the same note, lying above the pitch of either separately: this was with open pipes. I have observed a case where two stopped pipes "drew" together to a note below the pitch of either separately. This mutual influence also occurs with harmonium-reeds. With organ-pipes it presents an interesting problem of atmospheric vibrations. With harmonium-reeds it is practically important in connexion with the construction of tonometers. This subject remains almost unworked.

Loudness of Sound. Mechanical Equivalent of Sound.

The subject of the measurement of the loudness of sound will receive a new foundation in the admission of Fechner's psycho-physical law, with respect to the perception of sound by the ear*. This law is derived from the admission that equal fractions of any existing mechanical intensity produce equal impressions; and it results in the statement that the impression is the logarithm of the mechanical intensity.

Under these circumstances impressions have to be classified as to apparent loudness, in the same manner as stars are classified as to apparent brightness. The criterion of successive stages is that they appear equally distinct from one another when loudness only is considered. The following is a sketch of such a classification.

Audible sounds are divided into ten magnitudes. The first magnitude includes the loudest sounds. The magnitudes down to the fifth include lesser sounds that are still loud; the sixth to the tenth magnitude includes sounds that are not loud, the tenth magnitude containing the softest sounds that can be heard. The distinction between loud and not loud is very definite to my ear; it may not be equally so to others. Of course the following list represents only my own impressions, and it may probably require amendment when considered by others. Each magnitude includes the sounds up to the next on the loud side.

* See Helmholtz, *Phys. Optik*, p. 312. 'Nineteenth Century,' July 1879, p. 166, Galton (where the law is called Weber's law).

The organ is much used in the description, as by reference to it magnitudes can be described in a way that is intelligible to a large number of persons. Stops of average voicing are to be understood, not in the swell-box unless stated. The sound is supposed to be heard in a church or hall of moderate size.

The estimate is to be formed purely as to loudness; and for this purpose it is advisable to compare unmusical noises.

Sound-
magnitudes.

Loud.	{	1. Steam whistle. Cannon close by. Church bells in the chamber.
		2. Tromba (tuba mirabilis). Sounds of (1) at a little distance. Loud bells at foot of tower.
		3. Full organ without tromba.
		4. Trumpet with diapasons. Singing or public speaking at the top of the voice.
		5. Modern loud diapasons (German). Loud singing or intoning. Ordinary public speaking.
Not loud.	{	6. Soft diapasons (old English). Soft singing or intoning. Loudest ordinary conversation.
		7. 4 choir 8-foot stops. Ordinary speech.
		8. Stopped diapason alone. Soft speech.
		9. Dulciana. Strong whisper. Tick of watch close to ear.
		10. Dulciana or salicional (in swell-box closed). Faintest whisper. Tick of watch at arm's length.

Great precision is not attempted; but it is generally easy to say whereabouts in the scale a given sound lies. Precision will come in time.

Several problems then lie before us:—

(1) What is the common ratio of the mechanical intensity in two successive magnitudes?

(2) What is the absolute value of the mechanical intensity corresponding to one definite magnitude?

(3) What is the law of the dependence of the magnitude of sound of given mechanical intensity on variation of pitch.

As to the first two I have made some rough determinations; but the apparatus at my disposal is too imperfect to enable me to quote the results as being of any value. With a better bellows I see no difficulty in the way of answering these two questions*.

* Since the above was written I have made a determination of the ratio by observations of "Tom," Christ Church, Oxford, when the 101 strokes are rung, after 9 P.M. At the foot of the tower, say 30 yards off the source, it was of 2nd magnitude; at the distance of $1\frac{3}{4}$ mile, of 10th magnitude. This gives for the common ratio of the mechanical energy for two consecutive magnitudes, 1 : 3.2 nearly. The experiment with a resonator above referred to, gave 1 : 2.3 for the common ratio, from an estimated difference of two magnitudes, the estimate being of course very uncertain. The

As to (3), I showed some years ago* that, on certain suppositions which cannot be very far from the truth, the energy of notes of different pitch and the same loudness varies as the wave-length. The other conclusions drawn at that time were based on the supposition that mechanical intensity was a true measure of the impression on the sense. The arrangements now described will furnish the means of examining this point in other ways.

(4) When sounds of different pitch excite different parts of the sensorium, it appears probable that Fechner's law applies to each part separately. It is quite certain that a single soft stop sounding the octave below is detected at once, if added to the full organ without such stops; whereas the addition of a similar stop, having the same pitch as any part of the sound actually present, could not be detected by the most experienced ear. This part of the investigation is as yet untouched.

Phonograph and Phonautograph.

These instruments consist of devices for producing marks characteristic of sounds on a moving surface, generally a cylinder which rotates uniformly. The uniform motor will give to the results of these instruments a completeness which they now generally fail to possess. It has hitherto been almost impossible to obtain, for instance, phonographic records of musical sounds, on account of the uncertainty of the speed of rotation; and the exact reproduction of such sounds from the phonograph has presented great difficulties, if, indeed, it has ever been accomplished.

The most interesting applications of the phonograph, however, are to the analysis of speech. The forms corresponding to different vowels have been determined by Messrs. Jenkin and Ewing ('Nature,' xviii. pp. 167, 340, 394, 454). But the point in which the proposed arrangements will be of most value is in the analysis of the inflections of speech, or the rapid variations of pitch which occur continually. This analysis is of the highest importance for phonology, as the inflections are undoubtedly among the principal characteristics of dialects. The employment of the uniform motor in connexion with these recording instruments promises the easy solution of this problem.

observation of "Tom" is by no means final; besides the disturbing influence of buildings, &c., a light breeze got up at the time of the distant observation. But the determination seems worth quoting; indeed it cannot, I think, be very far wrong.

* Phil. Mag. 1872, xliv. p. 381.

Electro-pneumatic Clock Governor.

In all the applications of power useful for acoustic purposes, every thing turns upon the steadiness of the motor and its accurate regulation. I propose to indicate in outline how the pneumatic and electro-pneumatic apparatus in use among organ-builders will furnish convenient means of automatic regulation by the clock.

A good clock will close an electric circuit at every beat of the pendulum for a time which must not be too short. This current will communicate with an apparatus such as is employed in the electric action of the organ, in which air is admitted from a reservoir to a small power-bellows on closing the circuit. In this way a ratchet-wheel will be pushed forward a step every second. This drives a bevel wheel on the same axis. Another bevel wheel opposite, moving freely on the same axis, is turned in the opposite direction by the machine to be regulated. A third bevel wheel, with movable axis at right angles to the first axis, gears in the other two wheels. If the two other wheels move in opposite directions with equal speed, the third simply turns round on its axis. If either of the first two goes quicker or slower than the other, the axis of the third moves with half the differential velocity. If this axis be attached to the governing arrangement of the motor, the whole number of revolutions of the machine performed in any length of time can be constrained to preserve any desired ratio to the movement derived from the clock.

The details would occupy more space than is desirable. I will only say that pneumatic apparatus can be freely used with advantage. The ordinary pneumatic key, connected with its work by flexible tubing, and touched by a stud on the spindle of any part of the machine to be controlled, forms a most valuable resource for automatic regulation.

Pneumatic Motors.

It will not be generally convenient to drive more than one machine at regulated speed from the same motor. For this and other reasons the employment of secondary motors, driven from the bellows, will probably be of advantage. The form I propose to give to these instruments is that of a three-crank shaft and flywheel, with three power-bellows attached to the three cranks. They will also be controlled by the clock governor, with the assistance both of governed supply and pneumatic brake. For the finer regulation of speeds I anticipate that the best results will be thus obtained.

Bellows of Precision.

The only bellows of precision that I know of is that of Cavaillé Coll*: it is expensive; the complete machine costs £80. I will state shortly the principles on which the obtaining of steady wind from the feed depends; it will appear that it is not necessary to increase largely the cost of the ordinary bellows.

The simplest form of supply is one feeder driven by the up-and-down motion of a handle, the stroke overcoming also the weight of the feeder. This arrangement discharges a volume of air into the reservoir with a uniform velocity, which begins and ends suddenly: the weight of the feeder always causes a shock at the beginning and end of the stroke. The area of the feeder is generally equal to the area of the reservoir, so that the velocity imparted by the stroke to the top board of the reservoir is half that of the lift of the end of the feeder. This is the worst form of supply; it is common in very small organs.

In all cases the reservoir must be made with one inverted and one direct rib. This is well understood by organ-builders.

The next best form of feed is that common in English organs. Two equal feeders, each occupying half the area of the reservoir, are moved in opposite directions by up-and-down strokes of a handle. The feeders balance and the shock is materially lessened. The velocity of the upper board is a quarter the lift of the end of the feeder. But the stroke is still generally made with uniform velocity, beginning and ending suddenly.

The next improvement is the application of a pair of cranks, axle, and fly-wheel to the previous combination. In this case there are two discharges for each turn of the fly-wheel, whose velocities follow the pendulum-law. The velocity alternates between zero and its maximum. Great smoothness is here attainable so long as the speed is low; but at high speeds the variation of the velocity of supply will be objectionable.

This inconvenience may be to a great extent obviated by using three feeders instead of two, the three feeders being worked by three cranks on a shaft, set at angles of 120° . The velocity of supply in this case has maxima and minima at intervals of 30° ; but these maxima and minima are in the ratio of $2 : \sqrt{3}$, or $1 : 0.866$ nearly. This variation is less than that obtained by using four feeders, but it is more frequent. The value, however, is so nearly uniform that it is not thought that any considerable gain would result by increasing the number. If more exact uniformity were desired, six feeders at intervals of 60° would give exceedingly small variations of

* *Comptes Rendus*, 1863, i. p. 339. 'Nature,' xviii. p. 381.

the total velocity of supply, which would be absolutely the same at points 30° apart. The weights of the feeders being borne by the cranks, it is of no consequence that they do not absolutely compensate each other.

The escape-valves open into the feeders in the best modern work; the supply then ceases without noise or shock when the bellows is full.

The principle here applied is that of making the feed of wind itself steady; the plan more commonly adopted has been to employ appliances to overcome the effect of the unsteadiness of the supply.

The arrangement is, I believe, not new; but I do not know of any particular instance where it is in use. It is suitable for the employment of power. Governing arrangements can be applied.

It would be desirable that a lathe should form part of the laboratory fittings. With this assistance the more expensive and novel forms of apparatus might be constructed in the laboratory itself—such as new forms of the siren, oscillating disks, revolving stopcocks, clock governors, &c.

The most suitable engine that I have seen for the purpose is Rider's hot-air engine. This is worked by a certain definite mass of air alternating between a hot and cold cylinder, the two plungers working in cranks at right angles. It is the most silent engine that I have seen; and the smallest size of $\frac{1}{4}$ H. P. can be worked with a gas-burner. Independently of its suitability for laboratory purposes, it is an extremely pretty bit of practical thermodynamics.

So far as I am aware, no laboratory has been fitted with arrangements of the nature of those I have described. The plan seems to me worth trying; and I hope before long to make an effort to carry it out.

The cost of the whole of the new apparatus is hardly likely to amount to that of a set of tuning-forks of König and a "soufflerie de précision" of Cavaillé Coll together. It seems likely to do away with the need of a great part of the expensive apparatus formerly required for these purposes; but I do not suppose that it will be desirable to be altogether without the older apparatus. The experiments of König, for instance, can hardly be said to be repeated unless they are repeated to some extent under the original conditions; and the comparison of different methods may be expected to lead to instructive results. Electromagnetic forks are unquestionably of great importance, and for some purposes cannot be replaced, though for large departments of work we may with advantage find substitutes for them.

If such a laboratory should be fitted up, it would probably be contemplated that instruction should be given, ultimately at least, as well as research undertaken. But the *locale* which would be sufficient for research would not necessarily be suitable for lecturing or other instruction.

XXXVI. *Influence of Atomic Weight.* By THOMAS CARNELLEY, D.Sc., Assistant Lecturer on Chemistry in the Owens College*.

THE object of the present paper is to point out the influence which the atomic weights of the elements have on the chemical, and especially the physical, properties of both elements and compounds.

As early as 1826, Gmelin (and subsequently Pettinkofer, Dumas, Kremers, Gladstone, Cooke, Low, Odling, Fleay, &c.) directed attention to some curious relations between the atomic weights of certain classes of elements and also between their properties. Many of such relations will at once suggest themselves. Thus, of the elements Cl, Br, and I, bromine stands almost midway between chlorine and iodine, both as regards its atomic weight and its chemical and physical properties.

	Atomic weight.	Specific gravity of the liquid elements.	Melting-point†.	Boiling-point†.	Heat evolved by union with one atom H.
Cl =	35.5	1.33 at 15°	193	240	23783
I =	127	4.00 at 107°	387	473	— 3606
Mean of Cl and I =	81.2	2.66	292	356	10088
Br =	80	2.99 at 15°	248	331	9322

Very similar relations exist between Ca, Sr, and Ba, of which Sr holds a position almost intermediate between the other two; but what has been said with regard to Cl, Br, I, will be quite sufficient to illustrate the kind of relations which were pointed out at the time referred to. It was not, however, till within the last fifteen years that these relations were first traced in a systematic manner; and it is to Newlands, and especially to Mendeljeff, that we owe a new field of research and a new and powerful method of attacking chemical problems. The importance of the work of Newlands and Mendeljeff cannot be easily overrated. The principle proposed independently by each of them will serve in the future, and has done to some extent already, to indicate those directions in

* Communicated by the Author.

† Reckoned from absolute zero -273.

which research is most needed and in which there is most promise of interesting results. The application of this principle will also enable us to make predictions of phenomena still unknown, and will at the same time prevent many fruitless researches. It is and will be, in fact, for some time to come the finger-post of chemical science.

Notwithstanding the importance of this subject, it has up to the present been very much neglected. For though Mendeljeff's results are well known, yet the details and the service which his Periodic Law offers in the prosecution of chemical research are far from being so. Were his memoir more generally read, and the methods he proposes more widely applied, many fruitless researches would be avoided, and many important problems would be solved far more readily than is generally the case at present.

Up to within the last few years the work of the chemist has been very largely to collect facts. Now, however, the great tendency is to generalize, connect together, and offer some explanation of these facts. Thus, take a given element. Why should it possess certain properties and another element certain other properties? And what connexion is there between the different properties of the same element? What, for instance, constitutes the true difference between Ag and Cl, and why are they different? Why should silver melt only at 1000° C. and be a heavy solid at the ordinary temperature, whilst chlorine is a gas, very much less dense than silver? Why should silver have a great affinity for bromine, whilst chlorine has but very slight affinity for it? The greatest and highest object of the chemist and physicist, therefore, is to endeavour to offer some explanation of these and of a host of similar phenomena, and to get as near as possible at the root of the matter. In 1864 Newlands made the first great step in advance, which advance was increased and placed on a firmer basis by Mendeljeff in 1869.

The stage at which we have now arrived, and at which work is still being carried on with ever increasing activity, is the tracing of the interconnexion of properties. For this purpose, what we have to do is to find what properties vary regularly with other properties. A well-known example of this is Dulong and Petit's law of "Specific Heats," according to which the specific heat of an element is inversely as its atomic weight; also Gay-Lussac's law of vapour-density, which states that the vapour-density of a gas = $\frac{1}{2}$ its molecular weight. Also, I have recently shown (*Deut. chem. Ges. Ber.* xii. p. 440, March 1879) that the greater the coefficient of expansion of the elements by heat the lower the melting-point. And more

recently M. Raoul Pictet (*Compt. Rend.* lxxxviii. p. 855, April 28, 1879) has fixed this relation still more exactly; for he concludes:—(1) The higher the melting-point of a solid, the shorter are the oscillations of its molecules. (2) The melting-points of solids correspond to equal lengths of oscillation; and therefore the product of the length of oscillation by the melting-point is constant for all solids. Or, expressed in the form of an equation,

$$t \times \frac{\alpha}{\sqrt[3]{\frac{d}{p}}} = c;$$

where t = melting-point, reckoned from -273 , α = coefficient of expansion, d = specific gravity, p = atomic weight, and c = constant.

If we can prove that one or more certain properties all vary regularly with a certain other property, and if we can offer some explanation of this last property, we are in a fair way of being able to explain the other properties. Thus it has already been pointed out that, the higher the melting-point of an element, the less is its coefficient of expansion; and it can be easily shown that both these phenomena are dependent on the attraction between the molecules. It is generally allowed that in the solid condition the forces which draw the molecules together are greater than those which tend to drive them apart. In the liquid condition the opposing forces are more equally balanced, and the molecules move freely over one another. In the state of gas, on the other hand, the repulsive force is stronger than the attracting, and the body fills the whole space in which it is placed. If, then, a substance melts when the repulsive force = attracting force (and the repulsive force, we know, is due to heat), then it follows that the greater the attraction between the molecules of a body the greater will be the heat required to melt it. Consequently, if we compare a number of bodies at the ordinary temperature, their melting-points will be the higher the greater the attraction between their molecules, because the greater the heat required to overcome that attraction. We may therefore say that the melting-point of a body is proportional to the attraction between its molecules, when there is no repulsive force acting, *i. e.* at the absolute zero of temperature. If this be true, then we can easily see that the melting-point of a substance ought to bear some relation to its coefficient of expansion, hardness, &c., since these properties are evidently dependent on the attraction between their molecules; for the greater the attraction between the molecules of a body, the greater will be the heat required

to drive the molecules apart, and therefore the less will it expand on being heated; and consequently the greater the coefficient of expansion the lower the melting-point. The two phenomena of fusion and expansion by heat thus admit of a common explanation, viz. the attraction between the molecules. Again, the greater the attraction between the molecules of a body the greater the *external* force required to separate them mechanically, and therefore the harder the body. If this be true, then the hardness of a body ought to bear some relation to its melting-point. We actually find that, as a general rule, the harder a body the higher its melting-point: thus diamond, steel, &c. have a very high melting-point and great hardness, whilst lead, potassium, sodium, wax, &c., have a low melting-point and are soft. This relation, however, is not so regular and easily traced as in the case of the melting-point and coefficient of expansion, the reason being that the separating force (viz. heat) in the case of fusion and expansion is exerted *internally*, and therefore more uniformly than in the case of determining the hardness of a body, for in the latter the separating force is exerted *externally**.

Quincke ('Watts's Dict.' vii. p. 241) has shown that the order of capillarity of metals in the solid state is the same as their order of hardness; and this is what we might expect. Bettone (Pogg. *Ann.* cl. p. 644) has determined the hardness of the elements by finding the time required for a steel drill to penetrate to a certain depth, and has by this means shown that the hardness of an element $= \frac{\text{sp. gr.}}{\text{at. wt.}} = \frac{1}{\text{sp. vol.}}$, the following being a few examples out of a large number given in the original memoir:—

	Hardness.	$\frac{1}{\text{spec. vol.}}$
Diamond	0·301	·292
Iron	0·137	·137
Copper	0·136	·136
Platinum	0·109	·111
Zinc	0·108	·108
Silver	0·099	·096
Tin	0·065	·062
Lead	0·057	·055
Potassium	0·023	·022

* The first part of the present paper was written in the summer of 1878, and read before the Owens-College Chemical Society, January 1879, since when an article by F. Mohr, "On Cohesion," has appeared in Liebig's *Annalen*, cxcvi. p. 194 (1879), in which he points out that the metals form a "scale of hardness agreeing closely with the order of their melting-points."

Now it has already been pointed out that the harder a body the higher is its melting-point; and as the hardness likewise varies inversely as the specific volume, therefore the greater the specific volume of an element the lower ought its melting-point to be; and this we actually find to be the case *with the elements taken as a whole*. This will be again referred to further on, in speaking of Meyer's "Curve of the Elements." As the tenacity of a metal (or the weight required to break a rod of unit cross section) depends on the cohesion* between the constituent particles of the metal, we should expect that those metals which have the highest melting-points would also be the most tenacious; and this is really the case. Metals like Fe and Cu, which melt at comparatively high temperatures, have a far greater tenacity than such metals as Zn, Pl, Sn, which have but little tenacity.

We have thus endeavoured to show how many of the physical properties of the elements are interconnected with one another.

One of the chief objects of the chemist and physicist of the present day is to refer all the properties of the elements, both chemical and physical, to as few, what we may call, *standard properties* as possible, till finally one standard property is obtained, to which all the others may be referred in some way or other; or, in other words, *we have finally to choose some standard property, of which all the others are a function*, so that when we are able to explain this standard property we shall at the same time be able to arrive at the cause of the other properties, and thus be in a condition *to predict* the nature and degree of the properties of any given unknown element, or any unknown properties of a known element, of which the standard property has been determined numerically.

Now, whenever possible, we should select as our standard properties those which can be represented numerically, as the atomic weight, specific gravity, melting-point, boiling-point, &c.; and then of these take as our final standard property that which can be determined and represented numerically in the most exact manner, and which is subject to least variation with external circumstances. Thus, we might take as our ultimate standard the coefficient of expansion, since this is capable of pretty exact determination; but it would not be advisable to do so, as it is liable to great variation with the physical condition of the body. The tendency at present (and it is no doubt the right tendency) is to take the atomic weight as the ultimate standard, and refer all the other properties to it; for it is capable in most cases of very exact determination, as Stas's

* Mohr has recently shown that cohesion is but one form of chemical affinity, Liebig's *Ann.* cxcvi. p. 183.

classical researches have shown, and for a given element is, as far as we know, absolutely invariable.

This subject (viz. the reference of the properties of the elements to their atomic weights) was first attacked in a systematic manner by Newlands ("Law of Octaves," *Chem. News*, xiii. p. 113, also *ibid.* x. pp. 59, 194), who in 1864 pointed out that the atomic weights and properties of the elements vary periodically with one another; and in 1869 Mendeljeff, apparently independently of Newlands, in an elaborate and most important paper, propounded what is known as the *Periodic Law* (*Deut. chem. Ges. Ber.* ii. p. 553, *Ann. Chem. Pharm. Suppl.* viii. pp. 133-229). This law states that "*The properties of the elements are a periodic function of their atomic weights.*" What Mendeljeff in reality did was to take the atomic weight as his final standard, and represent each of the other properties as a function of this standard. Mendeljeff's researches refer chiefly to the relations between the atomic weights and properties of the elements; and before going on to the question of compounds, we shall direct attention to some of the more salient features of his work. He points out that if the elements be arranged according to the size of their atomic weights from $H=1$ to $U=240$, then the relations between their atomic weights and their chemical and physical properties exhibit a periodic function. As an example, take those elements the atomic weights of which lie between 7 and 36, thus:—

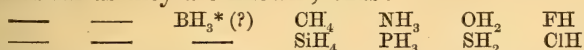
Li = 7	Be = 9.2	B = 11	C = 12	N = 14	O = 16	F = 19
Na = 23	Mg = 24	Al = 27.4	Si = 28	P = 31	S = 32	Cl = 35.5

Here the elements up to F are arranged in the first line and those after F in the second line, each element as it comes being placed immediately under the one above. It is readily seen that those elements which stand in the same vertical column have very similar properties, and are in fact always classed as belonging to the same family or group. It is therefore evident that the character of these elements changes regularly and gradually with the increase in their atomic weights; and that this variation is a periodic one, *i. e.* varies in the two series in a similar manner. As a further example of this, take the composition of the oxides of the above fourteen elements, and it will be seen that the corresponding members of both series form oxides having a similar composition; thus:—

Li_2O	BeO	B_2O_3	CO_2	N_2O_3	OO_2 (ozone)	F_2O (?)
Na_2O	MgO	Al_2O_3	SiO_2	P_2O_3	SO_2	Cl_2O

This shows also that those elements in the same vertical column have the same atomicity, and that the atomicity increases regularly up to the middle member of each series and

then diminishes in like manner. As a still further illustration of this, Mendeljeff cites the hydrogen-compounds of these elements as far as they are known; thus:—



Not only does the number of H-atoms vary regularly with the atomic weight, but the stability of these hydrogen-compounds under the influence of different agents, as well as their acid characters, and, in fact, all their properties, do likewise. Thus HCl is a powerful and very stable acid; H₂S is a weak acid and easily decomposed by heat; in PH₃ the acid characters are entirely lost and the stability very much diminished, and this still more so in SiH₄. The physical properties also of these elements vary periodically with their atomic weight, as in the case of the atomic volumes, which are as follows:—

Li = 11.9	Be = 4.4	B = 4.1	C = 3.6	N = ?	O = ?	F =
Na = 24.0	Mg = 14.0	Al = 10.0	Si = 11.0	P = 16.0	S = 16.0	Cl = 27.0

The atomic volume diminishes from the beginning to about the middle of each series, and then increases to the end of the series.

Such relations as have been described above apply not only to the fourteen elements which have been taken in illustration, but to all elements, as will be seen on consulting the following Table (p. 312), in which the elements are arranged in the order of their atomic weights.

This Table shows that the properties of the elements first change gradually with increasing atomic weight, and then on arriving at a certain point repeat themselves in a new series of elements or a *new period*. The change which takes place in the atomicity as the atomic weight increases is a very good example of this. The atomicity either increases up to the middle member of each series, and then diminishes regularly to the end, after which it begins to increase again in exactly the same way in the next series; or it continues to increase from the first to the last member of the series and then suddenly falls to unity on commencing with the next series. These facts are rendered evident by the numbers in the second horizontal line of the Table.

Again, those elements in the *same vertical* column belong to the same family or *group*, and of these groups there are eight; whilst those elements in the *same horizontal* line belong to the same *series*, and these series are at present twelve in number. On comparing the elements in any one group, we at once find that they are in each group divisible into two *subgroups*—and that in such a way that those members of the group belonging to *even* series are very nearly related to one

* Francis Jones, Chem. Soc. Journ. 1879, p. 41.

Natural Arrangement of the Elements.

Series.	Group I.	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.
Atomicity. }	Monads.	Dyads.	Triads.	Tetrads.	Triads or Pentads.	Dyads or Hexads.	Monads or Heptads.	
1.	H=1							
2.	Li=7·01	Be=9·2	B=11	C=11·97	N=14·01	O=15·96	F=19·1	
3.	Na=22·99	Mg=23·94	Al=27·3	Si=28	P=30·96	S=31·98	Cl=35·37	
4.	K=39·04	Ca=39·9	—	Ti=48	V=51·2	Cr=52·4	Mn=54·8	Fe=55·9; Co=58·6; Ni=58·6; Cu=63·1.
5.	(Cu=63·1)	Zn=64·9	Ga=69	—	As=74·9	Se=79	Br=79·75	
6.	Rb=85·2	Sr=87·2	Yt=89	Zr=90	Nb=94	Mo=95·6	—	Ru=103·5; Rh=104·1, Pd=106·2; Ag=107·66.
7.	(Ag=107·66)	Cd=111·6	Ir=113·4	Sn=117·8	Sb=122	Te=128?	I=126·53	
8.	Cs=133	Ba=136·8	Di=138	Ce=140	—	—	—	
9.	—	Ng=145·9*	—	—	—	—	—	
10.	—	—	Er=178	La=180	Ta=182	W=184	—	Ir=192·7†; Pt=196·7; Os=198·6; Au=196·2.
11.	(Au=196·2)	Hg=199·8	Tl=203·6	Pb=206·4	Bi=210	—	—	
12.	—	—	—	Th=231·5	—	U=240	—	

* The new element Norwegium, recently discovered by Dahll (Chem. News, 1879, xl. p. 25), appears to belong to this position.

† Seubert (*Deut. chem. Ges. Ber.* xi. p. 1767) has recently found the atomic weight of Ir = 192·7; and this agrees with the order of the coefficients of expansion, which increases from Ir to Au; and also with the order of the melting-points, which diminishes from Ir to Au (see Pictet, *Comptes Rendus*, lxxxviii. p. 1317); and also with the order of the specific gravity, which diminishes from Ir to Au (see recent determinations by Deville and Debray, *Comptes Rendus*, lxxxi. p. 839).

another, and those belonging to *odd* series also bear great resemblance to one another; whereas the corresponding members of even and of the following uneven series, though they have many points of resemblance, yet differ from one another in character much more than the corresponding members of two even or two uneven series. Thus, in group II., Ca, Sr, and Ba are nearly related, and also Mg, Zn, and Cd; but Ca, though in many respects it resembles Zn, yet it differs from it far more than it does from Sr or Ba. This theory of odd and even members of a group is well illustrated by the degree of reducibility of the elements to the free state,—those elements (as Ca, Sr, Ba, Ti, Zr, V, Cr, Mn, Ta, Nb, W, B, Be, &c.) belonging to *even* series being obtained in the free state with comparative difficulty, whereas elements belonging to *odd* series, as Cu, Ag, Au, Zn, Cd, Hg, Tl, Ir, Sr, Pd, Sn, Bi, &c., are easily reducible. I also hope to show shortly that all elements belonging to *even* series are *paramagnetic*, whilst those belonging to *odd* series are *diamagnetic*. We must therefore distinguish between Series and Groups, and between the even and odd members of a group.

We have also to distinguish between a *long period* and a *short period*. A short period contains but seven members; thus from Li to F and from Na to Cl form two short periods. A long period, on the other hand, embraces seventeen members: thus from K to Br and from Rb to I form two long periods; for we have to pass from K over sixteen elements before we come to one, viz. Cs, in which the properties of K are more or less repeated, whereas starting from Li, we have but to pass over six elements before we come to Na, or a point at which the properties of Li are in some measure repeated.

Another point to be noticed is that the last or seventh member of an odd series, as Cl, is very different, and in fact opposite in properties to the first member of the next even series, as K; whereas the seventh member of an even series, as Mn, has in many respects considerable resemblance to the first member of the next odd series, as Cu. Also in between the seventh member of an even series and the first member of the next odd series we have a somewhat peculiar group, viz. the eighth, in which there are three (or four) elements in each even series; and these elements stand midway in their chemical and physical properties between the preceding member of the seventh group and the first member of the next series. In other words, there appears to be an *abrupt* change to exactly opposite properties as we pass from the end of an odd series to the beginning of the following even series, as from Cl to K, Br to Rb, &c.; whereas we pass *gradually* from the seventh to the eighth groups of an even series, and thence to the first members of the following odd series.

This eighth group, as previously remarked, is a very peculiar group. The elements belonging to it resemble one another in many respects ; thus :—(1) they are all of a grey colour and are difficult of fusion ; (2) they possess in a high degree the power of condensing and giving passage to gases, as in the case of Pd, Pt, Fe, &c. ; (3) their highest oxides are bases or acids of little energy, and are easily reducible to lower oxides, which are far more basic ; (4) their salts form stable compounds with ammonia &c.

Atom Analogues.—From what has been said above, it will be seen that the position of an element R in the system of elements is determined by the series and group to which the element R belongs, and therefore by the elements X and Y standing on either side of it in the same series, and also by the elements R' and R'' standing above and below it in the same subgroup—thus,

			<i>Group.</i>	
			R'	
Series :—	X	R	Y	
		R''		

and this in such a way that the properties of R are the mean of those of X, Y, R', and R''. The elements X, Y, R', R'' are termed by Mendeljeff the atom analogues of the element R.

<i>Group VI.</i>				
			S	
Series 5:—	As	Se	Br	
		Te		

Here As, Br, S, and Te are the atom analogues of Se, the atomic weight of which is equal to the mean of those of the other four elements ; thus $\frac{75 + 80 + 32 + 128}{4} = 79$. So much,

then, for the influence of the atomic weights of the elements on their chemical properties. We shall now endeavour to point out this influence on the physical properties ; and for the sake of illustration we will take the melting-points, as they are capable of being represented numerically. In the following Table the elements are arranged exactly as in Mendeljeff's original Table (*vide supra*), the only difference being that the atomic weights are replaced, where possible, by the corresponding melting-points.

Table of the Melting-points of the Elements.

Series.	Group I.	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.
1. ...	H = gas		{ 'B melts at intense heat	{ C softens at the very highest temperature.	{ N = gas.	O = gas.	F = gas (?)	
2. ...	Li = 453	Be = b 1273	Al = 1123	{ Si = high temperature.	{ P (red = 528) (white = 317) } V does not melt at bright red heat.	S = 388	Cl = 193	
3. ...	Na = 369	Mg = 1023	—	{ Ti got only as a powder and not fused.	{ Nb got only as a powder and not fused.	{ Cr less fusible than Pt.	Mn = 2170	{ Fe = 2030; Co = 2070; Ni = 1870; (Cu = 1364).
4. ...	K = 335	{ Ca melts } above Sr	Ga = 303	—	As = a 773	Se = 490	Br = 248	
5. ...	Cu = 1364	Zn = 676	Yt = ?	{ Zr melts above Si.		Mo at a very strong heat.	—	{ Rh = 2270; Ru = 2070; Pd = 1970†; (Ag = 1273).
6. ...	Rb = 311	{ Sr melts } above Ba	In = 449	Sn = 503	Sb = 710	Te = 773	I = 387	
7. ...	Ag = 1273	Cd = 593	Di = ?	Ce = 710-1273	—	—	—	{ — — — (— — —)
8. ...	Cs = ?	Ba = 748	—	—	—	—	—	
9. ...	—	Ng = 527*	—	—	{ Ta got only as a powder and not fused.	W at a very intense heat.	—	{ Ir = 2770†; Os = 2770; Pt = 2270; (Au = 1520).
10. ...	—	—	Er = ?	La = ?	Bi = 538	—	—	
11. ...	Au = 1520	Hg = 233	Tl = 563	Pb = 605	—	U at a very high temperature.	—	{ — — — (— — —)
12. ...	—	—	—	Th = ?	—	—	—	

* Norwegium. Dahl, *Compt. Rend.* No. 18 (1879).These melting-points for the palladium and platinum groups have recently been published by Pictet, *Compt. Rend.* lxxxviii. p. 1317.

This Table shows that if the elements be taken in the order of their atomic weights there are, with but very few exceptions, no sudden jumps from a high to a low melting-point, or *vice versâ*, whilst the end of one series of elements runs gradually into the beginning of the next. Thus we pass by a series of gradations from easily fusible sodium to almost infusible silicon, and thence on to gaseous chlorine, then to easily fusible potassium, &c. This running of the end of one series into the beginning of the next is especially seen in the case of group VIII.; and it is worthy of note that a similar thing occurs with the heat of formation of the dichlorides, and also with the atomic magnetism of the same metals*, which latter has recently been determined by Wiedemann (Phil. Mag. [5], vol. iv. pp. 161, 276), thus:—

	Cr.	Mn.	Fe.	Co.	Ni.	Cu.	Zn.	Ga.
Melting-point a.	2270	2170	2080	2070	1870	1364	676	303
Heat of formation of dichlorides.	—	111990	82050	76480	74536	60988	(97200)	—
Atomic magnetism	—	100·4	83·1	67·2	30·5	10·8	—	—

The above remarks will be quite sufficient to show that this Periodic Law is a truly scientific one, and not a mere numerical curiosity; for it opens up new analogies, and therefore points out new paths for the investigation of the elements.

Numerical Relations between the Atomic Weights of the Elements.—Some interesting and curious *numerical* relations likewise exist between the atomic weights of the elements, of which the following may be taken as examples.

(1) The atomic weights of the elements of the first group are simple multiples of 7·7, thus:—

		True atomic weight.	
Li ...	$7\cdot7 \times 1 =$	7·7	
Na ...	$7\cdot7 \times 3 =$	23·1	
K	$7\cdot7 \times 5 =$	38·5	
Cu ...	$7\cdot7 \times 8 =$	61·6	
Rb ...	$7\cdot7 \times 11 =$	84·7	
Ag ...	$7\cdot7 \times 14 =$	107·8	
Cs ...	$7\cdot7 \times 17 =$	130·9	
?	$7\cdot7 \times 20 =$	154·0	[atom-analogues.
?	$7\cdot7 \times 23 =$	177·1	Calculated from the atomic weights of its
Au ...	$7\cdot7 \times 26 =$	200·2	Ought to be 199" according to Mendel"jeff.

Here the multipliers of 7·7, with the exception of those for Li, Na, and K, form an arithmetical series with the common difference 3, whilst between Li and Na, Na and K the common

* This was first pointed out by the author in a paper read before the Royal Society, June 19th, 1879 (Proc. Roy. Soc. No. 197, 1879).

difference is 2. Now it is remarkable that this difference of 2, instead of 3, for Li and Na likewise has its influence on the properties of these elements and those belonging to the same series; for, as Mendeljeff has pointed out, his second series of elements, containing Li, Be, C, N, O, and F, and part of his third series, containing Na, Mg, Al, and Si, are apparent exceptions to his theory of odd and even series. Thus Na, though belonging to the same subgroup, does not so much resemble Cu, Ag, and Au as the latter resemble one another; and the same thing holds good for Mg, F, O, N, C, &c. This is rendered especially evident by the melting-points (see Table on p. 315), and explains why N O, F?, Na, Mg, Al, and Si have melting-points which are so very different from those of the other members of their respective subgroups, and resemble more those of the other subgroup.

(2) Another interesting numerical relation between the atomic weights of the elements is that pointed out by Woehler (*Deut. chem. Ges. Ber.* xi. 11). This is represented in the following Table:—

	<i>a</i> .	$a + (1 \times 16)$.	$a + (2 \times 16)$.	$a + (3 \times 16)$.
Univalent	F = 19	Cl = 35.5	—	—
Bivalent	O = 16	S = 32	—	—
Trivalent	N = 14	P = 31	—	—
Quadrivalent...	C = 12	Si = 28	—	—
Trivalent	B = 11	Al = 27.4	—	—
Bivalent	Be = 9.2	Mg = 24	Ca = 40	—
Univalent	Li = 7	Na = 23	K = 39.1	—

$a + (4 \times 16)$.	$a + (5 \times 16)$.	$a + (6 \times 16)$.	$a + (7 \times 16)$.	$a + (8 \times 16)$.
Br = 80	—	—	I = 127	—
Se = 79	—	—	Te = 128	—
As = 75	—	—	Sb = 112	—
76	—	—	—	Ce = 140
—	Yt = 89	—	—	Di = 138
—	Sr = 87.5	—	—	Ba = 137
—	Rb = 85.5	—	—	Cs = 133

This Table exhibits the following relations:—

(a) *The affinity of the elements diminishes from F to Si with rising atomic weight and rising atomicity; and then from this point up to Cs it increases with rising atomic weight and falling atomicity.*

We have therefore F at the beginning and Cs at the end of the series; and these two elements have the strongest, but opposite, affinities. The other elements have a smaller affinity the nearer they stand to the middle of the series. This middle point is marked in the Table by the number **76** in large type. In other words, as we pass from F, the most negative of the elements, they tend to become less negative and more positive till we reach Cs, the most positive of all.

These statements are rendered evident by the following Table (p. 318) (given by Woehler in the memoir referred to),

	HF.	HCl.	HBr.	HI.	OH ₂ .	SH ₂ .	SeH ₂ .	TeH ₂ .	NH ₃ .	PH ₃ .	AsH ₃ .	SbH ₃ .	CH ₄ .	SiH ₄ .
F ...	—	?	?	?	?	?	?	?	?	?	?	?	?	?
Cl ...	—	—	Balard.	Gay-Lussac.	Rose.	Rose.	?	Davy.	Simon.	Balard.	Stromeyer.	Vogel.	Davy.	Fehling.
Br ...	—	—	—	Balard.	Löwig.	Rose.	?	?	Balard.	Balard.	Simon.	Simon.	Dalton.	?
I ...	—	—	—	—	Gay-Lussac.	Rose.	?	?	Balard.	Thomson.	Soubeiran.	Simon.	?	?
O ...	—	—	—	—	—	Davy.	Davy.	Davy.	Berzelius.	Davy.	Dumas.	Thompson.	?	Fehling.
S ...	—	—	—	—	—	—	?	?	Fourcroy.	Thomson.	Gay-Lussac.	Vogel.	?	?
Se ...	—	—	—	—	—	—	—	?	?	?	?	?	?	?
Te ...	—	—	—	—	Not Regnault.	—	—	—	?	?	?	?	?	?
N ...	—	—	—	—	—	—	—	—	—	?	?	?	?	?
P ...	—	—	—	—	Not Regnault.	—	—	—	Fourcroy.	—	Gay-Lussac.	?	?	?
As ...	—	—	—	—	Not Regnault.	—	—	—	—	—	—	?	?	?
Sb ...	—	—	—	—	Not Regnault.	—	—	—	—	—	Gmelin.	—	?	?
C ...	—	—	—	—	Not Berzelius.	—	—	—	Clouet.	—	—	—	—	?
Si ...	—	—	—	—	Not Gmelin.	—	—	—	—	—	—	—	—	?

Decomposes

in which is represented the affinity for H (which may be considered as a highly positive metal in the gaseous state) of the various elements from F to Si; and it is seen that these affinities diminish from F to Si in such a way that each element is capable of decomposing the hydrogen-compounds of all the succeeding elements.

(b) If the statement contained in (a) be true, *then the heat evolved (for 1 atom Cl) in the combination of the different elements with Cl to form normal chlorides will increase from F to Cs*, the heat evolved on the combination of 2 elements being a measure of their affinity for one another. This is represented in the following Table*:

Univalent	F = ?	Cl = ?	—	—
Bivalent	O = -7.6	S = ?	—	—
Trivalent	N = ?	P = 25.2	—	—
Quadrivalent...	C = ?	Si = 39.4	—	—
Trivalent	B = 36.1	Al = 52.0	—	—
Bivalent	Be = ?	Mg = 75.5	Ca = 84.8	—
Univalent	Li = 94.0	Na = 97.7	K = 104.6	—
Br = ?	—	—	I = ?	—
Se = ?	—	—	Te = ?	—
As = 24.0	—	—	Sb = 30.4	—
76	—	—	—	—
—	Yt = ?	—	—	Di = ?
—	Sr = 92.3	—	—	Ba = 97.1
—	Rb = ?	—	—	Cs = ?

(c) *The arithmetical mean of the atomic weights of the two elements with equal but opposite affinities is approximately equal to 76.* Thus:—

$$\frac{F = 19 + Cs = 133}{2} = 76, \quad \frac{O = 16 + Ba = 137}{2} = 76.5,$$

$$\frac{N = 14 + Di = 138}{2} = 76, \quad \frac{I = 127 + Na = 23}{2} = 75,$$

$$\frac{Te = 128 + Mg = 24}{2} = 76, \quad \frac{Sl = 122 + Al = 27.4}{2} = 74.5.$$

(d) *The melting- and boiling-points of the elements increase from F to Si with rising atomic weight and rising atomicity; and from that point to Cs they diminish with rising atomic weight and falling atomicity.* (Compare with Table on p. 315.)

In his original paper, Woehler shows that similar relations hold also as regards the specific heat and specific gravity of these elements.

* The numbers given in this Table are chiefly those determined by Berthelot and Thomsen, and were not given in Woehler's original paper.

W. C. Williams and T. Carnelley (Chem. Journ. 1879, p. 563) have shown that a curious relation (whether accidental or not it is impossible to say) exists between the melting- and the boiling-points of Cl, Br, I, and of S, Se, and Te. The melting-points of the latter elements are respectively twice and the boiling-points three times as high as those of the first-named elements, all being reckoned from the absolute zero (-273°), thus:—

Melting-points.

Cl = $198 \times 2 = 396$ (Berthelot)	S = 388 (Person).
Br = $248 \times 2 = 496$ (Baumhauer)*	Se = 490 (Hittorf).
I = $387 \times 2 = 774$ (Stas)	Te = 773 (Watts's Dictionary).

Boiling-points.

Cl = $240 \times 3 = 720$ (Regnault)	S = 720 (Regnault).
Br = $\left\{ \begin{array}{l} 318 \times 3 = 954 \text{ (Löwig)} \\ 331 \times 3 = 993 \text{ (Andrews)} \end{array} \right\}$	Se = 953 (Williams and Carnelley).
I = $473 \times 3 = 1419$ (Stas)	Te boils below 1660, since Deville and Troost took the vapour-density of Te at this temperature.

The melting-point of I is equal to that of S, and double that of Cl. That of Te is double that of S. Similar relations also hold in the case of the boiling-points, except that the boiling-point of I is two thirds instead of equal to that of S. If the boiling-point of I be twice that of Cl ($=240^{\circ} \times 2 = 480^{\circ}$), then the number 473 is 7° too low; but Stas gives the boiling-point of I as slightly above ($473 - 273 = 200^{\circ}$), thus confirming the calculated number.

Applications of the Periodic Law.—Mendeljeff has pointed out several important applications of the periodic law, of which the following are the more important:—

(1) *To the Classification of the Elements.*—This is in fact the only scientific and natural classification; for it is the only one which takes into account not only the chemical properties and atomicity, but likewise the physical properties and atomic weights.

(2) *To the Determination of the Atomic Weights of Rare Elements.*—This will be best explained if we take an example in illustration, such as indium. Having found the equivalent of the element ($=37.6$), we calculate what its atomic weight would be supposing it were a monad, dyad, triad, and tetrad respectively, and we obtain the following results:—

	Monad.	Dyad.	Triad.	Tetrad.
Atomic weight =	37.6	75.2	112.8	150.4

Now it cannot be a monad, because there is no vacant place

* Baumhauer states (*Deut. chem. Ges. Ber.* iv. p. 927) that Br always gives a melting-point two or three degrees too high if it is not perfectly dry.

in the Table of atomic weights (p. 312) corresponding to a monad with atomic weight approximating to 37·6, for they are already occupied by K and Cl. For similar reasons it cannot be a dyad; it must therefore be either a triad, and belong to group III., or a tetrad and belong to group IV. But in the latter case its atomic weight ought, according to the mean of the atomic weights of the atom-analogues of a tetrad position, to be either 140 or 164, and not 150·4; whilst its properties and those of its compounds ought to resemble either those of Sn and Pb or those of Zr and Th; but this is not the case. Consequently there only remains the triad position in group III. and atomic weight 113.

(3) *To the Determination of the Properties of still undiscovered Elements.*—The Table of elements (p. 312) shows that there are still many vacant places to be filled by elements yet to be discovered. But from what has been already said, it is easily seen that the periodic law renders it possible to predict, within certain limits, the properties of still undiscovered elements when those of their atom-analogues are known.

At the time when Mendeljeff published his memoir, the position between Al and In was still unoccupied; and from the properties of the atom-analogues of that position, viz. Al, In, and Zn, he made predictions as to the properties of the missing element. This then unknown element he termed eka-aluminum. Since that time gallium has been discovered, and its properties described by Lecocq de Boisbaudran, from which properties it appears that it is the missing element eka-aluminium. In order to show, therefore, the extent to which the predictions of Mendeljeff have been verified, the predicted and the actual properties of gallium are placed side by side in the following Table* :—

<i>Predicted Properties.</i>	<i>Actual Properties.</i>
(1) Atomic weight = 68.	(1) Atomic weight = 69·8.
(2) Specific gravity = 5·9.	(2) Specific gravity = 5·956.
(3) Easily fusible.	(3) Melts at 30° C.
(4) Its chloride will be volatile.	(4) Gallium chloride is easily fusible (melting-point 73° C.) and volatile.
(5) Will be easily obtained in the metallic state.	(5) Can be easily obtained in the metallic state.
(6) Its sulphide will be insoluble in water.	(6) Gallium sulphide is insoluble in water.
(7) Will be discovered by the spectro-scope.	(7) Discovered by spectroscope, giving characteristic violet bands.
(8) Precipitated by BaCO ₃ in the cold.	(8) Precipitated by BaCO ₃ in the cold.
(9) Oxide soluble in ammonia.	(9) Oxide partially soluble in ammonia.
(10) Will form an alum.	(10) Forms an alum.

(4) *To the Correction of those Atomic Weights which are*

* See also a paper by Muir (Phil. Mag. April 1877).

somewhat uncertain.—As a case in point, we have uranium, to which the atomic weight 120 was formerly assigned. Mendeljeff, however, pointed out in his original memoir that the true atomic weight of this element ought to be 240; and this number is now generally adopted.

(5) *To the Completion of our Knowledge of the Combination forms of Chemical Compounds.*

Meyer's Curve of the Elements.—In December 1869, shortly after Mendeljeff proposed his periodic law, Lothar Meyer published a very interesting article in Liebig's *Annalen* "On the Nature of the Chemical Elements as a Function of their Atomic Weights," in which he showed that by taking the atomic weights as abscissæ and the corresponding atomic volumes as ordinates, a curve is obtained which is a visible representation of the fact that not only the atomic volume, but also all the properties of the elements are a periodic function of their atomic weights. As the atomic weight increases, the atomic volume increases and decreases regularly in such a way that the curve which represents these periodic changes is divided by 5 maxima into 6 divisions, each of which has the form of a suspended chain. Of these divisions, the 2nd and 3rd, and likewise the 4th and 5th, are very similar to one another, and are of nearly equal dimensions. The 6th division is at present incomplete. The 2nd and 3rd divisions correspond to Mendeljeff's short periods, viz. Li to F and Na to Cl, and the 4th and 5th divisions to two of his long periods, viz. K to Br, and Rb to I.

As a general rule, those elements standing in corresponding parts of the curve have similar properties. In divisions 2 and 3 the atomicity increases regularly as we pass from left to right down the curve till we come to the lowest point, where the atomicity attains a maximum; and then, on rising up on the opposite side of the curve, the atomicity gradually decreases again till it becomes unity. In the other divisions of the curve the atomicity increases as we pass down the curve from left to right, but in this case comes to a maximum half-way down the curve, after which it decreases again, and attains a minimum at about the lowest point; and then as the curve rises again, the atomicity undergoes exactly the same changes as it did on the falling portions of the curve. Metals which are easily expanded by heat lie at or near the maxima, whilst those with a small coefficient of expansion stand at or near the minima. Heavy brittle metals occur just before the minima on the falling portions of the curve. All those elements which occur on the rising portions of the curve are brittle non-metallic elements.

In divisions 2 and 3 the elements on the falling portions of the curve are electro-positive and electro-negative, on the rising parts, whilst in divisions 4, 5, and 6 those elements following directly *after* the maxima and minima are positive, and those occurring immediately before these points are negative. A *well-marked* positive or negative character is possessed only by those metals which have comparatively large atomic volumes, or, according to Meyer, "the aggregation of a large mass in a small space appears to be incompatible with the development of a marked positive or negative character."

On comparing the Table of melting-points (p. 315) with Meyer's curve of the elements, we find that the melting-points (and also the boiling-points) rise and fall as the curve falls and rises; *i. e.* they are inversely as the atomic volumes*. The only important exceptions to this rule are As and Se in series 5, Sn, Sb, and Te in series 7, and Tl, Pb, and Bi in series 11, the melting-points of which are too high to be in accordance with the above rule. I have pointed out (*Deut. chem. Ges. Ber.* xii. 440), as previously mentioned, that the coefficients of expansion of the elements are the greater the lower their melting-points, and that there are but five exceptions to this rule, viz. As, Sb, Bi, Te, and Sn. Now it is seen that all these five elements are also among the eight exceptions to the relation between the melting-point and atomic volume, as is shown in following the course of Meyer's curve. All these eight exceptions likewise belong to *odd* series and follow one another directly in their respective series. A further inspection of Meyer's curve shows that it rises regularly and somewhat sharply from Ni, Cu, and Zn to Ga, and then, though it does not really do so, yet it shows a very strong *tendency* to turn downwards to As. In the same way, in division 5 the curve rises regularly and rapidly from Pd, Ag, and Cd to In, and then exhibits a strong downward tendency towards Sn and Sb. These facts may perhaps serve to explain why the melting-points of the elements As, Se and Sn, Sb, Te at these points on the curve are not regular either in relation to the atomic volume or to their coefficients of expansion. The same thing applies also in the case of Pb. Therefore, though at first sight these exceptions appear to contradict, yet a closer inspection shows that they rather confirm the course of Meyer's

* This is only what we might expect from the remarks on p. 308 with regard to Bettone's experiments. For since, according to him, the hardness of an element = $\frac{1}{\text{atomic vol.}}$, and since the harder a body, the higher its melting-point, then the greater the atomic vol. of an element, the more easily fusible it is.

curve, its downward tendency at the points referred to being merely the expression of the facts.

By taking the atomic weights of the elements as abscissæ and the reciprocals of the corresponding melting-points as ordinates, a curve is obtained which has a form similar to that of Meyer's (in which the ordinates represent the atomic volumes); for, as in the latter, we have at the maxima the halogens and alkali-metals, and at the minima C, Si, Ti, Mn, Zr, Pt, and allied elements.

Mechanical Arrangement of the Elements.—If the elements are arranged in the order of their atomic weights so that the first member of each horizontal series follows directly after the last member of the preceding series, B after Be, Al after Mg, &c., and if, with Lothar Meyer (*Die modernen Theorien der Chemie*, p. 301, 2nd edition), we imagine that the Table is rolled round a perpendicular cylinder in such a way that the group beginning with B and Al joins on to that of the alkaline-earth-metals Ba, Mg, Ca, &c, then we obtain, as is readily seen, a continuous series of all the elements arranged in a spiral according to the size of their atomic weights. Those elements which in this arrangement stand directly under one another on the cylinder belong to the same group. It is thus evident that by arranging the elements in this mechanical manner we obtain a truly natural scientific classification.

[To be continued.]

XXXVII. *Experimental Researches on the Temperature of the Sun.* By F. ROSETTI, Professor of Physics in the University of Padua*.

THE question of the effective temperature of the sun has been keenly discussed of late years, although as yet without any definite result. Newton was the first to attack this problem; it was afterwards studied by Saussure, and more recently by Pouillet, Waterston, Secchi, Ericson, Vicaire, Violle, Crova, and others.

Although the observations on radiation made by these investigators were tolerably concordant, the conclusions at which they arrived as regards the temperature of the sun are widely different. Thus Newton, Waterston, Ericson, and Secchi affirm that the temperature of the sun cannot be less than from one to two million degrees; on the other hand, Pouillet, Vicaire, Violle, and others maintain that it is not above 1500° , or at the very most 2500° .

* Translated from the *Ann. de Chim. et de Phys.* t. xvi. (1879) by John I. Watts, Owens College.

In 1876 the French Academy of Sciences offered a prize for the best solution of the question ; but although, at the termination of the competition, a prize was awarded to M. Violle, and encouragements to MM. Vicaire and Crova, the Commission of the Academy declared that the problem had not been solved *. The Commission pointed out that the chief source of error was the necessity of making use of an untrustworthy extrapolation in order to deduce the solar constant to the limits of our atmosphere, and that even this was not the least danger which necessarily follows from extending a law of radiation, which is hardly applicable to temperatures between 0° and 300° , to temperatures above the melting-point of platinum. Thus, from almost identical observations on solar radiation, Secchi obtained over $2,000,000^{\circ}$ by deducing the temperature from Newton's formula, whilst M. Violle obtained only 1500° by employing that of Dulong and Petit ; nevertheless it has been shown that both the formulæ are only applicable when there is only a very small difference between the temperature of the hot body which radiates and that of the cold body which is warmed by the radiation. When the two formulæ are applied to the case of a body rendered incandescent by the oxyhydrogen blowpipe, the temperature of which is certainly as high as 2000° , Newton's gives $45,900^{\circ}$, which is excessively high, whilst that of Dulong and Petit gives 870° , which is certainly too low. M. Violle, in order to justify the low value given by Dulong and Petit's formula, attributes the error to the emissive power of the incandescent body ; and, assuming the formula to be exact and applicable even in this case, he deduces from it an exceedingly small value for the emissive power.

It appears to me that, instead of forcing the formulæ to show what they can never do, it would be much better to confront the question directly, and, by means of well-chosen experiments, to ascertain the law according to which the intensity of radiation varies when the temperature changes, to determine the emissive power of the bodies on which the experiments are made and under the conditions in which they are at the time of the experiment, and, after having established the formula which expresses the radiation within the limits of the experiments which have served to fix it, to determine exactly whether it is applicable to the cases of higher and well-known temperatures. It is only when this correspondence exists that the use of the formula can be further extended, and the temperatures of inaccessible and exceedingly hot bodies like the

* *Comptes Rendus*, vol. lxxxvi. p. 813 (1877).

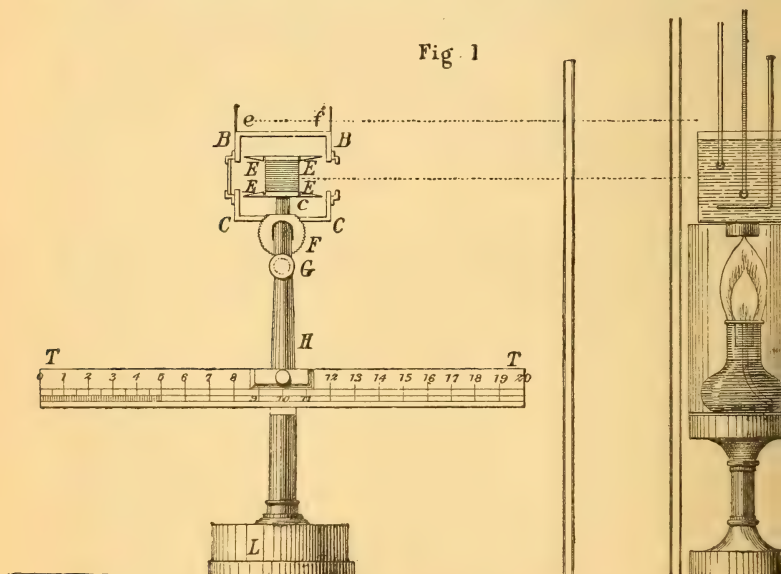
sun investigated with any degree of accuracy. In so doing I hope to overcome one of the chief difficulties which the problem presents. Like my predecessors, I have been obliged to have recourse to an extrapolation in order to obtain the value of the solar constant within the limits of our atmosphere; but the method which I have followed renders it much more precise, and I think that the numbers which I have obtained represent the temperature of the sun in a satisfactory manner. This will be rendered evident by a perusal of the present memoir.

I. *Description of the Instruments employed in the Researches.*

1st, *Thermoelectric Piles.*—In my experiments I have used two piles—one made by M. Duboscq, and the other by M. Gourjeon.

The one made by M. Duboscq (No. 1) was composed of 25 antimony-bismuth elements arranged in the form of a straight prism with a square base; the junctures of the metals were placed at the base of the prism, and were covered with lampblack. The first rod of bismuth and the last of antimony were in communication with two insulated coils of wire in which the rheophors were fixed. The pile was protected by a small brass case with double wall (fig. 1) in which were two apertures

Fig. 1



opposite to two sides of the pile. The pile was situated in the centre of the case and was kept in position by a piece of

metal, C. The case was in the form of a straight prism with a rectangular base. Each base, back and front, was movable, and had in its centre a window, which admitted the rays as far as the junctures in the pile. These windows could, however, be closed by double shutters working in horizontal grooves set in the outer wall of the case. The interior walls of the case were covered with lampblack; they could not, however, radiate towards the faces of the pile, because the latter were protected by two tubes EE , EE (rectangular in section), which were slightly divergent towards the exterior and reached nearly as far as the windows of the case. The case with the pile could be inclined more or less to the horizon by means of a toothed pinion, G , worked by a rack, F , fixed on the case. This rack was supported by a pillar, H , which was movable along a divided scale, T , and could be fixed in any convenient position by means of a clamp-screw. The pile could thus be moved in two directions—one along a horizontal line, the other about a horizontal axis. A third movement, of rotation about a vertical axis, could be obtained by turning the foot L by hand. For the purpose of accurately arranging the pile so that its two faces should be perpendicular to the rays, two sights (e and f) were fixed to the top of the case. The line passing through two holes made in these sights was parallel to the longitudinal axis of the pile. In employing solar radiation, it was certain that the faces of the pile were perpendicular to the rays of the sun when the pencil of light which passed through the hole in the front sight fell on the hole in the hinder one.

The thermoelectric pile made by M. Gourjeon (No. 2) was only used in a few special researches. It was composed of a great number of bismuth-antimony elements arranged in the form of a straight cylinder with a circular base. It was more sensitive than pile No. 1.

2nd, *Galvanometer*.—An excellent Wiedemann's galvanometer measured the electric current generated in the pile when exposed to radiation. In this galvanometer the deviations of the magnet were read by means of a telescope on a divided scale situated below the telescope and some distance from the needle. The current which deflected the magnet traversed a wire which was wrapped on two horizontal bobbins, which were placed on the two sides of the magnetic disk; they were insulated from one another, and could be moved either backwards or forwards. On each of the bobbins there were wound two wires, in order that the instrument might be used as a differential galvanometer. I arranged the connexions so as to make the electric current travel over the two

wires of the same bobbin in the same direction. Moreover the two bobbins were placed as near the magnetized disk as possible; and in addition I placed a strongly magnetized bar underneath, for the purpose of neutralizing as completely as possible the influence of the earth's magnetism on the disk. In this manner the instrument was rendered extremely sensitive. The telescope (furnished with cross-wires) and the divided scale were placed at a distance of 3 metres from the galvanometer. The scale was divided into fifths of a centimetre, and by means of the telescope could be read to the tenth of a millimetre. When the readings of the variations were made, it was necessary to pay attention to the differences in the position of equilibrium of the disk, due to variation of the magnetic declination of the earth; on this account, before passing the current, the division of the scale corresponding to the cross-wire of the telescope was noticed; and this served as a starting-point in the elimination of successive variations.

II. *Experimental Determination of the Law of Radiation as a function of the Temperature.*

Experiments between 0° and 300°.—In order to make this determination, I took first for radiating body Leslie's cube filled with water, which I heated to different temperatures by a flame, and finally kept it at the boiling-point. For temperatures between 100° and 300° I employed an iron cube filled with mercury. The cover of this cubical vessel was pierced with three holes, through two of which passed the stems of two thermometers, each of which was graduated up to 360°; through the third the handle of an iron stirrer passed. At a short distance from the radiating face of the cube was placed a diaphragm with a double wall; in the centre of each wall were circular holes with a diameter slightly less than that of the opening in the case containing the pile. This precaution was taken to insure that every part of the pile should receive an equal amount of heat. The exterior walls of the screen were blackened; and between the pile and the first screen a second, also with double sides, was placed with windows slightly larger than those in the case. The two screens were placed parallel to each other and to the face of the case by means of small holes, in the same manner that the pile was directed towards a radiating object. The experiments were made with the more sensitive pile No. 2, and with no more resistance in the circuit than that offered by the pile itself, the rheophors, and the wire of the galvanometer. The following Table shows the results of the experiments:—

TABLE I.

Surrounding temperature 23°·8 C.

Number of experiment.	Temperature of the radiating face.	Difference between temperature of radiating face and surrounding temperature.	Deviations observed on the galvanometer.
1.	56·6	32·8	10·0
2.	96·6	72·8	29·5
3.	116·6	92·8	42·8
4.	136·6	112·8	55·0
5.	156·6	132·8	72·5
6.	176·6	152·8	91·5
7.	196·6	172·8	116·7
8.	216·6	192·8	141·9
9.	246·6	212·8	169·5
10.	256·6	232·8	204·0
11.	276·6	252·8	239·5
12.	296·6	272·8	283·5

It should be stated that each experiment was repeated at least twice, and that the necessary precautions were taken to keep the temperature constant during the experiment.

III. *Empirical Formulæ, and the Choice of the one which best represents the Phenomenon.*

It was important to determine from these experiments the law according to which the thermal effect produced by radiation varies with the temperature of the radiating surface. For this purpose I constructed a curve from the preceding Table, taking for abscissæ the numbers contained in the third column, and for ordinates the corresponding numbers of the fourth column. A simple inspection of the curve showed that the thermal effect of the radiation increases much more rapidly than the temperature of the radiating surface, and therefore Newton's law, which is expressed by a straight line, is not applicable here. In fact it has long since been shown that Newton's formula is only applicable when there is a small difference between the temperature of the radiating body and the body which receives the radiation. By a very careful series of experiments Dulong and Petit proved that the law of radiation is represented by the formula

$$q = b(a^{t-t_1} - 1);$$

in which q represents the quantity of heat given out by the unit surface of the radiating body in the unit of time,

t the temperature of the hot body,

t_1 the surrounding temperature,

a and b two constants.

Although this formula correctly represents the phenomenon of radiation in the experiments of Dulong and Petit, which extended only between the limits of 0° and 280° , yet it has been shown by Ericson that it is not applicable when the difference in temperature between the radiating body and the surrounding temperature exceeds 80° . M. Jamin declares that it is an empirical formula which becomes inexact at high temperatures. Since, then, the formula of Dulong and Petit is empirical and limited in its use, I have sought to substitute for it another formula, which more correctly represents the radiation as a function of the temperature of the radiating body and the temperature of the medium surrounding the cold body which becomes warm. After much consideration I have decided to adopt the formula

$$y = aT^2(T - \theta) - b(T - \theta);$$

in which y represents the thermal effect of the rays, measured by the thermoelectric pile ;

T is the absolute temperature of the radiating body;

θ is the absolute temperature of the medium in which the pile is placed ;

a and b are two constants to be determined, which depend on the nature of the thermoelectric instrument, and which remain constant for one and the same body radiating at all temperatures.

The first of the two terms may be regarded as representing the thermal effect produced by the rays *in vacuo*; the second represents the influence of the surrounding air. This formula is, as regards its form, identical with that of Newton, since the value of y is proportional to the difference $T - \theta$; but whilst in Newton's formula the emissive power of the radiating body is considered to be independent of the temperature, in the formula proposed by myself the emissive power is represented by ET^2 ; that is, it is proportional to the square of the absolute temperature of the radiating body. In a body with a maximum emissive power, such as lampblack, E should be equal to 1 only for $T = 1$; but as T increases the emissive power should also increase, in proportion to the square of the temperature. This supposition was confirmed by many of my

experiments, and also by those of Tyndall on the emission of heat (see Pogg. *Ann.* vol. cxxiv; also Wüllner, *Lehrbuch der Physik*, vol. iii. pp. 215, 216). The best experiments of Melloni and Tyndall have shown that, as the temperature rises, the radiation of the body increases, because not only does the energy of the rays belonging to the primary undulations increase, but also new undulations of higher refrangibility are added to them. Thus the effect of radiation increases on account of the very large number of rays of different refrangibility, and on account of the intensity of each ray.

In order to ascertain whether the formula properly represents the phenomenon of radiation between the limits of my experiments, and to find out whether it is capable of further extension, I began by determining the value of the constants a and b by means of the experimental data of the preceding Table. By taking experiments 7 and 10, in the first of which

$$y=116.7, \quad T=196.6+273=469.6, \quad T-\theta=172.8,$$

and in the second

$$y=204.0, \quad T=256.6+273=529.6, \quad T-\theta=232.8,$$

I obtained the values

$$\log a=4.5252152-10, \quad a=0.00000335131,$$

$$\log b=8.8040253-10, \quad b=0.0636833.$$

In order to make sure whether the formula, with the values of a and b calculated in this way, represents the radiation for all differences of temperature between 0° and 273° , the values of y for intervals of 50° were calculated and compared with the corresponding values taken on the curve.

TABLE II.

Differences of temperature. $T-\theta$.	Temperature observed on the thermometer, C. t .	Absolute temperature. T .	Ordinates y taken from the curve.	Ordinates y calculated by the formula.	Difference.
0	23.8	296.8	0		
50	73.8	346.8	17.2	16.97	+0.23
100	123.8	396.8	46.4	46.40	0.00
150	173.8	446.8	90.1	90.80	-0.70
200	223.8	496.8	151.7	152.69	-0.99
250	273.8	546.8	234.7	234.58	+0.12
272.8	296.6	596.8	279.6	279.25	+0.35

I wished to determine to what extent the formula of Dulong and Petit was capable of representing the effect of radiation

between the limits of the experiments made. For this purpose the two values,

$$T - \theta = 130^{\circ}, \text{ to which } y = 69.9 \text{ corresponds,}$$

$$T - \theta = 260^{\circ}, \quad ,, \quad y = 253.5 \quad ,,$$

were deduced from the curve and introduced into the formula

$$y = b(a^{T-\theta} - 1).$$

In this manner the values

$$b = 42.9728, \quad \log b = 1.6331939,$$

$$a = 1.00746, \quad \log a = 0.00322612,$$

were obtained for a and b . The following Table shows the differences between the ordinates deduced from the curve and the values calculated from the formula of Dulong and Petit.

TABLE III.

Difference of temperature. $T - \theta$.	Ordinates taken on the curve.	Ordinates calculated by Dulong and Petit's formula.	Difference.
50	17.2	19.23	-2.03
100	46.4	47.35	-0.95
150	90.1	87.98	+2.12
200	151.7	146.88	+4.82
250	234.7	232.27	+2.43

Therefore the parabolic formula which I have proposed is more suitable than that of Dulong and Petit; and accordingly I have used it in my later experiments.

[To be continued.]

XXXVIII. Notices respecting New Books.

Researches on the Motion of the Moon, made at the United States Naval Observatory, Washington. By SIMON NEWCOMB, Professor U.S. Navy. Part I. Reduction and Discussion of Observations of the Moon before 1750, forming Appendix II. of the Washington Observations for 1875. Washington, 1878.

THE object for which this work was undertaken is fully expressed in the first paragraph of the author's preface as follows:—"For several years after the publication of Hansen's Tables of the Moon it was very generally believed that the theory of the motion of that body, after having been the subject of astronomical and mathematical research for two thousand years, was at last complete, and that in consequence the motion of the moon could now be predicted

with the same accuracy as that of the other heavenly bodies. In 1870 the writer showed that this belief was entirely unfounded, and that the correctness of the tables since 1750 had been secured only by sacrificing the agreement with observations previous to that epoch, so that about 1700 Hansen's tables deviated more widely from observations than did those which they superseded. Altogether it appeared that, notwithstanding the immense improvement that Hansen had made in the accuracy of the inequalities of short period, the theory of those of long period was no nearer such a solution as would agree with observation than when it was left by Laplace."

In consequence of Hansen's tables deviating so widely from observation in 1700, and generally at epochs previous to 1750, "the latter date was fixed upon as the terminal point of the investigation, consisting of the study of the inequalities of long period, partly because it is the epoch at which accurate meridian observations commence, and it is also that which separates the period within which we have readily accessible observations and copious tables of reduction founded on modern data from that during which both these requirements are wanting."

This investigation, as well as that of the mathematical theory of the inequalities of long period in the moon's mean motion, were made a part of the author's official duty at the Naval Observatory. Difficulties arising in the investigation of inequalities of long period, it was found necessary to leave this part incomplete until the best method of treating the subject could be decided on. In the meantime the author, in the course of a journey in Europe, obtained several valuable series of observations which he has employed in obtaining his result. He remarks that the material most used has hitherto been least known; also that the most valuable portion of it is possibly found in the unpublished Paris observations, whereby the moon's mean longitude is determined with astronomical accuracy from 1680 onward.

The general scope of the work may be gathered from the table of contents, as follows:—§ 1. Historical Introduction. § 2. Summary of Data for determining the apparent Secular Acceleration. § 3. Discussion of Narratives of ancient total Eclipses of the Sun. § 4. The Ptolemaic Eclipses of the Moon recorded in the *Almagest*. § 5. Arabian observations of Eclipses, from Caussin's translation of *Ebn Jounis*. § 6. Mode of deducing the Errors of the Lunar Elements from observations of Eclipses and Occultations. § 7. Effect of Changes in the Lunar Elements upon the path of the central line of an Eclipse. § 8. Observations of Bullialdus and Gassendus. § 9. Observations of Hevelius. § 10. Observations by Astronomers of the French School between 1670 and 1750, from Manuscripts at the Paris and Pulkowa Observatories. § 11. Positions of the Moon from Hansen's Tables used in comparing the preceding observations with theory. § 12. Details of Reduction of the Occultations. § 13. Equations of Condition given by the preceding

Occultations of Stars. § 14. Observations of Eclipses from 1620 to 1724. § 15. Discussion of Deviations in the Moon's mean Motion. § 16. Motion of the Moon's node. § 17. Concluding Remarks on the Value of the Secular Acceleration deduced in this paper.

For the subjects treated in the sections above enumerated we must refer the reader to the work itself, in which will be found evidence of the skill with which the author has accomplished his task, and the results at which he has arrived. In section 15, devoted to the discussion of Deviations, the author, speaking of the cause of outstanding deviations, remarks that we may make two hypotheses:—(1) That these deviations are only apparent ones, arising from inequalities in the axial rotation of the earth. The deviation of the observed secular acceleration from the theoretical value $6''\cdot18$ has long been attributed to a retardation of the earth's rotation; and by supposing this retardation to be a variable quantity, and indeed sometimes to change into an acceleration, we may completely account for the observed deviations. (2) We may suppose the deviations to arise from one or more inequalities of long period in the actual mean motion of the moon. On the first of these hypotheses the author says, "If it is correct, the problem of predicting the moon's motion with accuracy through long intervals of time must be regarded as hopeless, since it cannot be expected that variations in the earth's axial rotation will conform to any determinable law. Success in tracing the deviations in question to the moon itself, and to the theory of gravitation, is therefore a consummation to be hoped for."

In the last section, containing the concluding remarks on the value of the secular acceleration deduced by the author, he suggests that "either the recently accepted value of the acceleration and the usual interpretation of the ancient solar eclipses are to be radically altered, the eclipse of -556 not having been total at Larissa, and that of -514 not having been total in Asia Minor, or the mean motion of the moon is, in the course of centuries, subjected to changes so wide that it is not possible to assign a definite value to the secular acceleration.

An important feature of this book consists of the collection of observations of eclipses and occultations (some hitherto unpublished) used by the author in his investigation. Altogether the work forms a valuable addition to the literature of the Lunar Theory.

XXXIX. *Intelligence and Miscellaneous Articles.*

ON A VISUAL PHENOMENON AND ITS EXPLANATION.

BY WILLIAM ACKROYD, F.I.C.

VISUAL phenomena are of general interest, and are often described but seldom explained. The phenomenon in question may be seen under the following circumstances. Face the breeze, and without winking allow a small raindrop to fall on the surface

of the cornea, all the while keeping your gaze fixed on a lamplight some hundred feet away. As the raindrop alights on the cornea, several rings of light appear to surround the luminous source. *The rings gradually contract in diameter.* Explanation:—In sunshine, the moving ring crest of water produced by dropping a pebble into a still and shallow pool projects a ring of light on the bottom which gradually increases in size. The moving ring crest, by its refractive action, produces a hollow cylinder of rays of ever increasing diameter; and we see a section of it on the bottom of the pool. The raindrop falling on the cornea spreads out on its surface in several ring crests, and would similarly produce a series of outward travelling rings of light were it not for the combined action of the refractive media of the eye. Under the influence of these, two hollow cones of light are formed within the vitreous humour directly upon impact of the raindrop. The first of these has for its base a small circular area of the hind surface of the lens; and its prolongation, the second cone, has the retina for its base. As any individual ring-crest spreads out on the cornea, the first cone increases in size, the common apex advances towards the retina, and consequently the section of the second cone projected onto the retina decreases in size and appears as a contracting ring of light.—*Abstract of paper read in Section D, British-Association Meeting, Monday, August 25th.*

PRÉCIS OF A REPORT ON ELECTRIC-LIGHT EXPERIMENTS.

BY L. SCHWENDLER, ESQ.*

Mr. Schwendler said it would be impracticable to read the "Précis of Report on Electric-Light Experiments" *in extenso*, since it contained too many technicalities which could not easily be followed if the paper were read in the manner usual at these meetings, and that he therefore would prefer to give verbally a short account of his researches and the results obtained. He stated that the inquiry originated with General Strachey, who, in April 1876, recommended to the Secretary of State that a trial of illuminating Indian railway-stations by the electric light should be made. In February 1877 Mr. Schwendler was requested to institute detailed inquiries, which led him to propose that it would be advisable to first make some more experiments before a practical trial at Indian railway-stations should be attempted. The Board of Directors of the East-Indian Railway Company agreed to this, and sanctioned the necessary outlay, whatever it might come to.

The experiments made at the India Office Stores, London, terminated on the 1st November, 1878. The report, however, could not be finished in time before Mr. Schwendler left for India; and he therefore prepared a précis—the paper before the meeting to-night. After pointing out the general results obtained, and explaining in a brief manner the three principal questions at issue,

* Read before the Asiatic Society 5th March 1879 (from the Proceedings for March). Communicated by the Author.

viz. *economy of the electric light, practicability and efficiency of the electric light for certain illuminating-purposes, and best means of distribution of the electric light*, Mr. Schwendler proceeded :—

You all have heard, no doubt, a great deal about the division of the electric light. During the last two years this question has been before the public almost permanently. This is not to be wondered at, if we consider that on the solution of this problem it will ultimately depend whether the new mode of lighting becomes a successful and general rival to the illumination by gas or other combustive means. But before entering on the subject, it will be best to formulate the question definitely, to avoid any misunderstanding with respect to the answer I am about to give. The question is:—A given permanent current (C), no matter how produced, does work in a closed single circuit of total resistance (R), of which a part (r) represents the resistance of *one* electric arc. This electric arc produces an electric light of *measured* intensity (I). Now, if we introduce, instead of *one* arc, *two* arcs of resistance r' and r'' and measured light-intensities i' and i'' respectively, and suppose the current to be the same as before, or the E.M.F. and total resistance in the single circuit the same, then *a priori* we should conclude that $I = i' + i''$ for $r = r' + r''$. Experiments, however, show that this not the case; *i. e.* the sum of the measured intensities of *two* small lights is perceptibly smaller than the measured intensity of *one* large light; and this difference becomes larger and larger as we increase the number of lights produced by the same current, *i. e.* by the same E.M.F. with the same total resistance in circuit. This appears at first sight an inconsistency with the known laws of *cause and effect*. How is it possible that the *same* current through the *same* resistance should produce more light in *one* point than in *two* points, although the total amount of work done by the given and constant current is exactly the same in *one* point as in *two* points?

That the *measured* intensity of *one* light is invariably greater than the sum of the measured intensities of n lights, is an undoubted fact proved by my own experiments very conclusively. But we may well ask, What has become of the energy which is expended and does not appear as light?

A careful analysis of all the physical facts connected with the subject will, however, show easily enough how this apparent loss of energy is to be accounted for, without reverting to far-fetched explanations, and without the necessity of making such statements as “the division of the electric light is in contradiction to dynamic principles,” or “the laws of nature must be reversed” (whatever that may mean), or “new laws have to be discovered first, before a solution of this important problem could be even attempted,” &c. &c., which I have read frequently in scientific or professional journals and newspapers. Statements of this kind appear very clever to the uninitiated, and they are exceedingly cheap to make; but they will invariably do an enormous amount of harm

to the further progress of an important application of the resources of nature.

It will be seen from the foregoing that I have called the light-intensity *measured* intensity. For if we produce a light by any source, it will be at once perceived that not *all* the light produced by that source can be made available for illuminating-purposes. A part of the total light will be lost for the special purpose of illumination, inasmuch as only a part of the total light is in a position to act on the photometer, or, which is the same, on the retina. Hence we may say the total light produced by any means consists of two parts: the one is lost for illuminating-purposes, and may be called *internal* light; the other acts on the retina, can be measured, and may be called *external* or *measured* light. For instance, of all the light produced in *one* electric arc, a considerable part is hidden by the electrodes between which the arc plays, because the electrodes have a volume, and moreover the positive electrode is hollowed-out like a dome, and it is the highest point of that dome which contains the most intense light, which is mostly lost. How much this loss in each case will be, depends on a variety of circumstances. In the first place, all other conditions being the same, that loss will increase with the thickness of the electrodes. The loss of light will further increase with a decrease of the length of the arc. By length of arc is to be understood the distance between the highest point of the hollow of the positive electrode and the apex of the negative electrode. Hence already in the case of *one* arc, although naturally we have here the longest arc for the given current and the given electrodes, the light lost or the *internal* light may represent a considerable portion of the total light produced in the arc.

If we produce *two* arcs, it will be seen *at once* that the sum of the losses must be greater than the loss in *one* arc. Hence the sum of the measured intensities of two lights must also be smaller than the measured intensity of *one* light. Suppose the length of *one* arc, when a given current passes, is 3 mm., then the sum of the lengths of *two* arcs will not be 3 mm. but much less, in order to have the same current passing through the *two* arcs as passed before through *one*. From this it follows that the loss of light must increase rapidly with the number of lights, and moreover that soon a limit for the possible practical division of the electric light is reached, leaving out the question of economy altogether.

This constitutes one of the reasons why the division of the electric light becomes less and less economical with increase of the number of lights, and that soon a practical limit will be reached for the division.

To express this result more definitely, we may say:—

The consumption of power per unit of *measured* or *external* light is a function increasing with the number of lights produced by a given current in a single circuit—supposing, of course, always that the sum of resistances of the n arcs is equal to the resistance of

one arc, and that the other resistance in the circuit, in which no light is produced, has remained constant throughout.

If we had a material infinitely conducting, of infinite strength, and with a melting point at least as high as that of carbon, then surely the division of the light would be perfectly economical up to any limit, inasmuch as we might then use linear electrodes.

In practice we can only try to approach this limit. Up to the present time there appears to be no better material for electrodes than carbon, either natural or artificial. But this is no reason why an effort should not be made to try to find a material for electrodes more accommodating to the division of the electric light than even carbon. The above, limited strength, limited electric conductivity, and limited melting-point of the material of electrodes, constitute only *one* of the difficulties which stand in the way of an unlimited economical division of the electric light.

A second cause is, for instance, the fact that in each arc an E.M.F. is established opposite to the original E.M.F. and by no means to be neglected against it. This secondary E.M.F. established in each arc appears to be a function of the current which passes the arc, most likely proportional to that current. Hence, if for a given current passing *one* arc this secondary E.M.F. be e , then the same current through n arcs, successively connected, would produce an E.M.F. equal to ne . This secondary E.M.F. ne is to be subtracted from the original E.M.F.; and, internal resistance of the original E.M.F. plus resistance of leading wires having remained constant, we necessarily have to decrease the total resistance of the n arcs in order to work with the same current as before. This merely means a decrease of the total length of the n arcs, or, which is the same, an increase of internal light or decrease of the *measured* or *external* light. A parallel connexion of the n arcs with reference to the poles of the given original E.M.F. would certainly produce only *one* secondary E.M.F. instead of n ; and for this reason it might be better to use the parallel circuit for the division of the electric light. But there are other very important objections to this solution. In the first place, as can be easily shown, the variation of *one* arc has a far greater influence on the variation of the others in parallel, than in successive circuit. Further the length of each arc must be made very much smaller in parallel circuit than in consecutive circuit.

Another reason against an unlimited economical division of the electric light is constituted by the practical necessity that lamps, of whatever construction they may be, have a resistance inherent to their nature in addition to the resistance of the arc. For instance, in an ordinary lamp with an electromagnet, the resistance of the lamp consists of the resistance of the electromagnet plus the resistance of the two electrodes when metallically closed. This resistance, although small, is by no means *nil*, and cannot be neglected against the resistance of the arc, especially when strong currents are used. In other words, when producing the electric light in n points instead of one point, we are unable to practi-

cally fulfil the condition, that the sum of the resistances of the n arcs is equal to the resistance of *one* arc, to have the same current; *i. e.* the former must be made smaller than the latter, on account of practical construction reasons.

We may therefore sum up as follows:—*The economical solution of the division of the electric light is theoretically quite possible, but practically difficult to obtain.* The division can never become unlimited; but ingenious inventors may nevertheless *solve the problem practically.*

The attempt by inventors to solve the question is therefore perfectly legitimate. If their attempt cannot lead to a perfect solution, they may nevertheless do so approximately, and by it tend towards real progress in illumination, inasmuch as by their attempts the electric light may probably become more and more a successful general rival to gas, which at present the electric light certainly is not.

Before I conclude, I must briefly avert to a paper on the Electric Light by Mr. W. H. Preece, published in the Philosophical Magazine for January 1879, in which the author believes that he has demonstrated from dynamical considerations that the division of the electric light is impossible. This it certainly is under the conditions introduced by Mr. Preece, *viz.* that the resistance of each voltaic arc, or each incandescent wire, is maintained *constant*. But it is unfair to the electric light to introduce this condition, especially as it does not at all represent the question at issue.

When a number of lights are connected in series, the resistance of each must be diminished, and when a number of lights are joined parallel, the resistance of each must be increased in proportion to their number, so as to maintain the total external resistance constant. If Mr. Preece will introduce this condition into his equation, he will find that theoretically the division of the electric light is quite possible, *i. e.* that, theoretically, however the lights be arranged, the unit of light will always be produced by the same expenditure of energy. Inventors should not, therefore, be down-hearted. On the other hand, investors in gas need not hasten to get rid of their shares; for there are many questions involving practical difficulties which still remain to be solved; but, at the same time, gas companies should be aware that they have a formidable rival in the field, and bestir themselves to maintain the lead they hold by improving their own means of illumination and extending its application.

THE TRUE THEORY OF FRESNEL'S INTERFERENCE PHENOMENA.

BY H. F. WEBER.

When the interference-fringes produced by a biprism or Fresnel mirror are examined, the effect observed in the focal plane of the eyepiece is the same as that which would result from two spherical waves emanating from the two images of the luminous source, and bounded respectively by the rectangular apertures occupying the place

of the two mirrors or the two halves of the prism. Fresnel (*Mémoires sur la diffraction*, vol. i. p. 355 *et passim*) assumes that, in the central part of the fringes, the diffraction resulting from this limitation of the waves plays but an absolutely negligible part. On that hypothesis the fringes should all have the same breadth, proportional to the distance from the focal plane to the luminous images, the minima of intensity should all be *nil*, and all the maxima equal, in homogeneous light, and in white light the central fringe should be of absolute whiteness.

According to M. Weber *, attentive observation shows that these consequences are not exactly verified: the relative breadths of the lines vary with the distance of the sources from the focal plane in which they are observed; the intensities of the maxima and minima differ very much; in white light the central line is nearly always coloured. M. Weber arrives at the explanation of all these appearances by taking into account the fact that the waves which produce the phenomenon are not indefinite. By calculations necessarily long, and into the details of which we of course cannot enter, he first reduces the problem to the determination of Fresnel integrals, which he afterwards expresses by means of a Bessel function and another, analogous, definite integral. He thus obtains a comparatively simple expression for the intensity of the light in any point of the interference-field.—*Bibliothèque Universelle, Archives des Sciences physiques et naturelles*, September 15, 1879, tome ii. pp. 360, 361.

AN ABSORPTION HYGROMETER. BY A. VAN HASSELT.

The air whose moisture is to be ascertained is introduced into a flask which is furnished with a thrice-perforated stopper. Through one of the perforations a glass tube is passed which reaches to the bottom, through the second a thermometer, and through the third a glass tube communicating, by means of an indian-rubber tube, with a manometer having oil for its liquid. Both the glass tubes can be closed by glass cocks. The indian-rubber tube is itself surrounded with a second one, filled with oil in order to prevent any diffusion of the aqueous vapour—a circumstance which Edelmann (*Wied. Ann.* v. p. 455) has not taken into consideration. It might probably be advantageous to substitute glass for the indian rubber. First, a thin-walled glass sphere containing anhydrous phosphoric acid is put into the flask, and is broken by shaking the latter. From the variations of pressure in the manometer the amount of aqueous vapour is determined. In order that the previous volume of the air may be restored, the manometer consists of two glass tubes connected by one of indian rubber. The results were perfectly satisfactory.—*Beiblätter zu den Annalen der Physik und Chemie*, 1879, No. 9, p. 697.

* *Vierteljahrsschrift der Züricher naturforschenden Gesellschaft*, 1879.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

NOVEMBER 1879.

XL. *On the Relation between the Thermoelectric Properties, the Specific Resistance, and the Hardness of Steel.* By CARL BARUS, *Ph.D., of Cincinnati*.*.

[Plate XI. figs 1-6.]

I. *Introductory Remarks.*

THE experiments which gave rise to the following paper were commenced with the view of further studying the relation between the maximum of permanent magnetism, hardness, and form of steel, a subject proposed for inaugural work by Prof. Kohlrausch.

Although this question had elicited considerable experimentation ever since Coulomb's time†, it was not until comparatively recently that harmonious results were arrived at, chiefly through the labours of Ruths‡, Rowland§, Gauguin||, Fromme¶,

* Communicated by the Author, having been read in extract before the Physico-Medical Society of Würzburg on the 18th of January, 1879.

† Coulomb, *Biot. Phys.* iii. p. 108, &c.; Hansteen, *Pogg. Ann.* iii. p. 236, 1825; Müller, *Pogg. Ann.* lxxxv. p. 157, 1852; Plücker, *Pogg. Ann.* xciv. p. 28, 1855; Wiedemann, *Pogg. Ann.* cvi. p. 169, 1859; Lamont, *Handbuch d. Magnet.* pp. 223, 249-253.

‡ Inaugural Dissertation, p. 34 (Darmstadt, 1874)

§ *Phil. Mag.* [4] l. p. 361, 1875.

|| *Compt. Rend.* lxxxii. p. 145, 1876.

¶ *Gött. Nachr.* Nr. 7, 1876, p. 157 *et seqq.*

Trève and Durassier*, and Gray†. All these observers, however, have classified steel, with reference to its hardness, either simply into hard and soft, or have accepted the colours of the oxide film of the tempered bar as a criterion of distinction sufficient for their purposes. It seemed, therefore, that the most probable method of further elucidating the magnetic subject referred to would consist in attempting to find some method by which the hardness of steel can be more *distinctly and more rationally* expressed. My endeavour was, in other words, to give the very vague notion hardness as applied to steel a *quantitative* signification. So long, however, as the ultimate nature of hardness does not admit of accurate definition, it is sufficient for the accomplishment of this end to examine some of the other properties of steel which likewise vary with its hardness, and by considering the magnetic moment, *cæteris paribus*, as dependent on the former, to eliminate, as it were, the notion of hardness between them. My attempt is, in short, to find an expression for the more complicated functions of hardness, *cæteris paribus*, in terms of the more simple. Of the latter the *thermoelectric properties* and the *specific resistance* of steel, both admitting of accurate and easy determination, appear most suitable.

As, however, the experiments on hardness and the electrical properties of steel alluded to, although only introductory in their character, gave rise to a number of new results, I determined to publish them separately. To obtain as complete a picture as possible of these phenomena, I have made free use of all the information on the subject within my reach. In each case the author borrowed from is cited.

II. Apparatus for Hardening Thin Steel Wire.

For reasons which become apparent below‡, the principal experiments of the following paper are confined to *thin* rods cut from the same coil. The rather difficult task of hardening these homogeneously throughout their length, without giving rise to a change in their chemical composition (either from oxidation or carburation), I believe I have accomplished by the aid of the following apparatus.

A glass tube 200 to 300 millims. long, 8 millims. wide, was provided at a distance of about 80 millims. from one end with two opposite apertures *aa* (Pl. XI. fig. 1), each about 3 millims. in diameter. This part was then surrounded by a cork A, perforated perpendicularly to the axis of the tube in a manner

* *Ann. de Chim. et de Phys.* (5) v. p. 266, 1875.

† *Phil. Mag.* [5] vi. pp. 321-323, 1878.

‡ Difficulties due to structure, *vide vii., d.*

to correspond with the holes *a a*. This arrangement is fastened vertically in a suitable iron stand (not shown in the figure). The wire to be hardened is introduced into the tube and fastened below to a brass rod *b h* fitting tightly in the perforation *b a a b*, and above to the spring *d K*. For the purpose of fastening the lower end, it was found sufficient, after having previously wound it round the rod *b h* so as to form a coil which could easily be made to slide off, to push the rod through *b a a b* and the coil, the latter having been introduced into the tube from the top. The spring *d K*, around the lower half of which the other end of the wire was wound, the upper half being provided with a clamp-screw *g g*, was fastened to a second arm of the stand (also omitted in the figure). By properly adjusting the rod *b h* and the arms of the stand, the wire could be brought into coincidence with the axis of the tube and stretched as far as was necessary.

A powerful galvanic current heated the wire to the degree of redness desired. The former entered at *g g* and passed back to the battery through *b h*, as shown in the figure. To prevent the oxidation of the wire during the process of heating, a current of dry CO_2 gas * was passed through the tube, entering by means of the hose C attached to the lower end.

After the wire had attained a steady uniform glow, the hose C was closed by the fingers, its connexion with the carbonic-acid apparatus disadjusted, the open end being connected with a neighbouring hydrant instead; hereupon the faucet of the latter was quickly opened, the galvanic current being at the same time interrupted: the water dashing up the tube with great velocity, imparts to the wire the hardness desired. Before each experiment the parts of the apparatus were well dried in a warm current of air.

The apparatus described presents the following advantages:—(1) By employing currents of different intensity, thus heating the wire to different degrees of redness, we are able to obtain corresponding degrees of hardness, which, though scarcely distinguishable mechanically (all appearing equally hard and brittle), have very different effects on the magnetic and electrical properties of steel†. (2) From the

* Having accidentally employed moist carbonic-acid gas, a small flame was observed at the top of the tube. This is probably due to the combustion of both H_2 and CO , the former being generated by the decomposition of aqueous vapour by the hot steel, the latter by the action of the nascent H produced on the Co_2 .

† See IX. The coercive force of steel being a minimum at a point in incipient redness, it is possible that the apparatus might be used in obtaining intense circularly or longitudinally magnetic wires. In the first

fact that the wire is kept in a state of continual tension by the spring d K, and from the particular method of chilling, the wires remain straight after being hardened. (3) The very slight oxidation noticeable on the hard wires is probably due only to the contact of hot steel and water in the act of hardening. Disadvantages, however, arise from the fact that the use of the apparatus is confined to thin bars, and that the wires obtained may be in a condition of circular magnetization. This would partly prevent their employment in subsequent magnetic experiments. The difficulty may, however, be avoided by breaking the galvanic circuit a little before opening the faucet.

III. *Methods of Measuring the Hardness of Steel Electrically.*

(a) *Thermoelectric Position and Hardness of Steel.*

In this place it will be expedient to leave the special consideration of steel for a moment, turning our attention to the electromotive force of a thermoelement composed of any two different metals A' and A'' .

Kohlrausch has shown* that the phenomena included under the head of thermoelectricity can be explained on the hypothesis that the heat-current is always accompanied by an electric current whose intensity is proportional to the number of caloric units passing the same section. He thus arrives at an expression for the electromotive force between any two metals (A' and A''), which, if for simplicity we suppose the cold end to be kept at zero †, has the following form:

$$E_{\tau} = (\mathcal{S}' - \mathcal{S}'')\tau(1 + f(\tau)),$$

where E_{τ} is the electromotive force corresponding to a difference of temperature τ of the ends, $(\mathcal{S}' - \mathcal{S}'')$ a constant specific for the combination.

This expression of Kohlrausch is very convenient, inasmuch as it allows us to separate the actual electromotive force into two terms, of which the first, $\mathcal{S}'\tau(1 + f(\tau))$, is dependent only on the metal A' and τ , the second, $\mathcal{S}''\tau(1 + f\tau)$, only upon A'' and τ .

Now we know that the thermoelectric position of a metal is

case the wires should be cooled without breaking the current, in the second the tube surrounded by a coil through which, during the act of hardening, a powerful galvanic current flows. See Holtz, *Wied. Ann.* vii. p. 71, 1879.

* *Pogg. Ann.* clvi. p. 601, 1875.

† The thermoelectromotive force being, according to Tait, Avenarius, Hankel, and others, a function of the temperatures of the two ends.

dependent not only on its *chemical nature*, but also on its *mechanical condition* (hardness). Let us therefore put

$$\mathfrak{S}' = \mathfrak{S}'_0 + \theta', \quad \mathfrak{S}'' = \mathfrak{S}''_0 + \theta'',$$

where $\mathfrak{S}'_0$ and $\mathfrak{S}''_0$ are to represent the (absolute) constants dependent on the chemical nature of A' and A'' respectively, θ' and θ'' , however, varying with the hardness of the metals. Thus the above equation becomes

$$E_\tau = \{(\mathfrak{S}'_0 - \mathfrak{S}''_0) + (\theta' - \theta'')\} \tau (1 + f(\tau)).$$

But suppose that A' and A'' are not different metals, but represent two rods to which different degrees of hardness have been imparted, which were originally, however, cut from the same wire. In this case $(\mathfrak{S}'_0 - \mathfrak{S}''_0) = 0$, whence

$$E_\tau = (\theta' - \theta'') \tau (1 + f(\tau)),$$

dependent only on τ and the difference of hardness of the rods.

It will be shown below that the electromotive force of an element of soft and hard steel varies continuously with the difference of temperature τ and with the difference of hardness of the rods. We will therefore put θ'' , the constant belonging to the soft bar (*i. e.* one which has been heated above redness and allowed to cool slowly in a badly conducting medium), equal to zero, as it is in this that the molecules will most probably have assumed normal positions. If, furthermore, we replace

$$E_\tau = \theta' \tau (1 + f(\tau)) \text{ by } E_\tau = a\tau + b\tau^2,$$

a sufficient approximation for practice, we derive

$$\frac{dE_\tau}{d\tau} = a + 2b\tau \text{ and } \left[\frac{dE_\tau}{d\tau} \right]_0 = a \text{ for } \tau = 0.$$

This expression, *i. e.* the limiting value of the electromotive force of a thermoelement composed of a soft rod and one of any degree of hardness to the corresponding difference of temperature* when the latter converges towards zero, will in the following be taken as the *measure* of the hardness of the harder bar. I shall apply the term *thermoelectric hardness* to it throughout the following paper (abbreviated T. E. H.).

The relation between the thermoelectric properties and hardness of steel, notwithstanding its comparative importance, has never, to my knowledge, been made the subject of detailed and exclusive study. All the experiments thus far

* The colder end being supposed at 0° .

published (the principal being those of Magnus*, Sir W. Thomson†, and E. Becquerel‡) are of a qualitative nature, the results being derived from the direction of the current observed on bringing together, in one way or another, wires of different hardness.

The experiments of Magnus being limited to hard-drawn wires, do not properly fall within the scope of the present paper. The same is true of a number of the experiments on steel in the excellent paper of Sir W. Thomson. With regard to the effects of annealing, Prof. Thomson observes:—"In cases of round steel wire, of steel wire flattened through its whole length by hammering, and of steel watch-spring, the thermoelectric effects of annealing portions after the whole had been suddenly cooled was a current from unannealed to annealed through hot." This result comprehends all that has thus far been done.

(b) *Specific Resistance and Hardness of Steel.*

With reference to the specific resistance and hardness of steel, we shall proceed in a manner analogous to the preceding. Denoting the observed specific resistance of a bar by S , that part of S which is due only to the *chemical* nature of the rod by S_0 , that due to its hardness by ΔS_0 , we have

$$S = S_0 + \Delta S_0.$$

Now, as it follows from results given below that the specific resistance of steel increases continuously with its hardness, it will be convenient to put ΔS_0 for a soft bar equal to zero. The value of ΔS_0 for a bar of any degree of hardness thus numerically determined will in the following be accepted as a second *measure* of that property.

The work thus far published on the relation between specific resistance and hardness of steel is due principally to Mousson§. Of late results have also been announced by Chwolson||. The data of both observers agree only qualitatively with mine.

IV. *Determination of Thermoelectric Hardness. Apparatus.*

Method.—In the determination of thermoelectric force the procedure known as Ohm's method was first employed. After-

* Pogg. *Ann.* lxxxiii. p. 486, 1851.

† Phil. Trans. iii. pp. 709-727, 1856.

‡ *Ann. de Chim. et de Phys.* [4] viii. p. 402, 1866.

§ *N. Denkschr. d. Schw. Gesellsch.* [8] xiv. pp. 1-90, 1855.

|| *Mél. Phys. de St. Pétersbourg*, x. p. 379, 1877. See also Carl's *Repert.* xiv. p. 15, 1878.

wards, however, it was found expedient to measure these forces (as Kohlrausch and Ammann * had done in similar experiments before) by a method of compensation, the object being to avoid the difficulties from the species of polarization due to Peltier's phenomena. The method can be easily deduced from that proposed by Bosscha, the latter, in the case where small electromotive forces are to be measured, admitting of simplification.

In the diagram (fig. 4), E denotes the compensating element (1 Daniell element, = 11.7 Weber-Siemens units), e the thermoelectric couple whose electromotive force is to be determined, both acting as shown in the figure, C a Weber's commutator (employed for reasons given below), G the galvanoscope. Let us represent the resistance of the branch ab by x , that of the branch aEb by $W+k$, where W represents the large resistance of a rheostat interposed, k that of the remainder of the branch (about equal to 1 Siemens unit) including E .

When the current in G is zero, we have

$$\frac{e}{E} = \frac{x}{W+k+x}.$$

But as the ratio $\frac{e}{E}$ is small, and therefore of necessity also x (maximum value = 10 Siemens units), in comparison with $W+k+x$ (about 20,000 Siemens units), we may with sufficient approximation put

$$e = \frac{E}{W} x,$$

the experimental accuracy obtainable allowing us to neglect $k+x$ in comparison with W . In the experiments the branch ab was a small Siemens rheostat.

The precise moment in which the current in the galvanoscope is equal to zero can be best determined by observing whether the needle on closing and opening the circuit remains at rest. This, however, is only possible when the opposed currents from e and E which pass through the galvanoscope are closed simultaneously. To accomplish this, the little cups at the end of the rods 1 and 2 of the commutator were quite filled with mercury, those of 3 and 4 only partially. By this device, on closing, C_1 and C_2 are first brought into contact, and the current $E C_2 C_1 b a W E$, not passing through the galvanometer, comes into action; in the next moment (C_3 and C_4 being joined) the current from e and the partial current from E referred to are closed simultaneously. In this way also induc-

* Pogg. *Ann.* cxli. p. 459, 1870.

tion-currents, which may possibly be generated in the rheostat, are without disturbing effect. The commutator merely serves the purpose of a double key.

As the electromotive forces measured were all very small, the large resistance W could be left unaltered, so that $e = \text{const. } x$. Now the resistance x was so chosen that the intensity of the current from e exceeded that of the partial current from E by the minimum possible. The thermoelectric force, however, decreasing with the temperature ($T - t = \tau$) of the ends, a moment soon arrives at which the intensities of the two currents are equal, and the deflection of the needle $= 0$ in consequence. At this point the thermometers are read off.

Thermoelement.—Instead of measuring the electromotive force of soft and hard steel directly, it was found expedient to compare all the rods with one and the same piece of copper wire. By this means the apparatus could be considerably simplified and many practical difficulties avoided. The construction of the copper-steel couple is given in vertical section in fig. 2.

To raise the ends of the steel rod to different temperatures, two doubly tubulated spherical receivers, each about 1 decim. in diameter, were used. These, held in position by movable supports of poorly conducting material, and so placed that the tubulures A and B were horizontal, the other two vertical, were connected by a glass rod cd fitting water-tight in the perforated corks adapted to the horizontal tubulures. This rod served a double purpose: by uniting the receivers as one, it prevented breakage of the very brittle steel rods, at the same time allowing them to be easily adjusted and removed; on the other hand, the receivers could by means of it be placed at any distance apart, this being necessary, as the rods to be examined were of very different lengths.

On one side of the glass rod the copper wires which acted as poles of the instrument were inserted once for all; on the other two appropriate holes served for the introduction of the steel rods $s s$ to be tested. The ends of the latter were connected with the corresponding ends of the copper wires by small flat clamp-screws of brass.

The apparatus being thus ready for experiment, the two receivers were filled with distilled water at T and t degrees respectively, where t was so chosen as to differ but slightly from the temperature of the room. The thermometers (introduced through the vertical tubulures) were read off with a telescope as follows: the deflection of the needle having become very small, t was determined, after which, when the current in G was $= 0$, T , whereupon another check-reading

of t was made. Before each observation, the water in the receivers was well stirred.

Galvanoscope.—The galvanoscope used was a very delicate instrument of Sauerwald, provided with mirror and astatic needle. The deflection of the latter was read off by mirror and scale. As the telescope of this instrument stood side by side with the telescope of the thermometers, both readings could be conveniently made by the same observer.

V. Determination of the Specific Resistance.

For determining the specific resistance of steel rods, use was made of a Wheatstone-Kirchhoff's bridge. An appropriate mercury commutator allowed the observer to interchange the unknown resistances without altering the value of those belonging to the bridge proper. Thermoelectric disturbances were avoided as far as possible by closing the current (one Smee with large resistance) only for very short intervals of time. Finally they were eliminated completely by replacing the hydroelectromotive force by a Weber's magneto-inductor*. The resistances of all rods were determined in terms of an arbitrary standard δ (0.0312 Siemens unit at 0°), chosen to correspond in magnitude with the unknown resistances. The galvanometer used was the one already mentioned above.

As the resistances to be measured were all very small (0.1 to 0.01 Siemens unit), great care had to be taken to exclude all disturbing resistances arising from insufficient contact. Soldering could not be resorted to, as it was believed that the ends would thereby have been annealed. The method adopted was as follows:—The ends of the rods having been well cleansed, were covered to about 1 centim. with a thin adhesive film of galvanically deposited copper, which was thereupon amalgamated (easily accomplished by plunging the freshly covered part in mercury). The rod thus prepared was then fixed, together with a glass rod in two corks, in a manner similar to that employed in the case of the thermoelement, and the whole, except the amalgamated ends, covered with a thick coat of varnish. Suitable wooden cups provided with horizontal and vertical apertures completed the connexion of

* Pogg. *Ann.* cxlii. p. 418, 1871. The use of the magneto-inductor in connexion with the bridge was suggested by Kohlrausch. This physicist also showed that this method is applicable even when the resistances to be determined are in the form of coils, the extra currents produced being calculable. I found the application of great convenience, inasmuch as the observer always has the needle of a delicate galvanometer completely under his control.

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the rods with their respective parts of the bridge by means of
mercury.

The efficient length was determined by deducting from
the total length that of the amalgamated ends. For the mea-
surement of the diameter, use was made of the microscope ;
a determination of this dimension from known length, weight,
and specific gravity was impracticable, inasmuch as the use of
the pycnometer, which alone would have given sufficiently
accurate results, would have compelled me to break the rods.

VI. *Experimental Results.*

A. *Thermoelectric Hardness of Rods suddenly immersed in Cold Water while in different states of Red Heat.*

The following (older) results were obtained directly by
Ohm's method. E_r was put = const. τ —a condition nearly
fulfilled by couples of soft and hard steel between 0° and 80° ,
in which case also the T. E. H. and the constant of propor-
tionality are identical. The rods were hardened in the appa-
ratus described in § II. ; diameter = 0.678 millim. ; the num-
bers preceded by the point (·) were afterwards checked by
the method of compensation. T. E. H. is expressed in Weber-
Siemens units.

The third column of the following Table contains the num-
ber of large Bunsen cells employed in the heating of the wire,
the fourth the observed degree of redness at the time of sudden
cooling.

TABLE I.

No.	T. E. H.	Bunsen Elements.	Remarks.
1	0.0000004		Soft, cooled slowly.
2	0.000003	4	Below redness.
3	0.000012	5	
4	0.000000	5	
5	0.000000	6	
6	0.000000	6	} Dark red.
7	0.000052	7	
8	0.000049	7	
9	0.000054	7	
10	0.000056	8	
11	0.000057	8	
12	0.000063	8	} Brick-red.
13	0.000064	9	
14	0.000065	9	
15	0.000064	9	

The Bunsen cups were introduced without altering the remaining parts of the circuit. The length of the wire heated was not in all cases the same.

B. The following determinations were made in the summer of 1878; temperature of the room very constant, and at about 20° , this, as already observed, being nearly the same as t , the temperature of the water in the colder receiver. The method of compensation was employed throughout. The determination of T was effected by a Geissler normal thermometer (graduated in $0^{\circ}\cdot 1$), that of t by an ordinary instrument (graduated in $0^{\circ}\cdot 2$), which had, however, been carefully compared with the former.

In Tables II. and III. the difference of temperature of the ends of the steel rod is given under τ , the corresponding electromotive force for the elements copper-steel under E_{τ} ; α and β are constants which satisfy the equation $E_{\tau} = \alpha\tau - \beta\tau^2$. These were calculated by first computing their proximate values out of two distant observations, and then adding to them corrections deduced from five of the most satisfactory observations by the method of least squares.

If, now, we denote by a and b the constants for an element of soft steel, hard steel corresponding to those α , β and α' , β' of the same rods when compared with copper, we shall have, since $E_{\tau} = \alpha\tau - \beta\tau^2$ and $E'_{\tau} = \alpha'\tau - \beta'\tau^2$,

$$E_{\tau} - E'_{\tau} = (\alpha - \alpha')\tau - (\beta - \beta')\tau^2.$$

But $E_{\tau} - E'_{\tau} = E_{\tau}$, the electromotive force of the element steel-steel; so that, since also $E_{\tau} = a\tau - b\tau^2$,

$$a = \alpha - \alpha',$$

$$b = \beta - \beta'.$$

The constants a and b are given in the last two columns. a may be regarded as numerically equal to the T. E. H. above defined, as E_{τ} is nearly a linear function of τ .

TABLE II.—Rods 0.678 millim. thick.

No.	τ .	E_{τ} , observed.	E_{τ} , calculated.	α .	β .	$a = T, E, H$.	b .	Remarks.
I.	13.92	0.001464	0.001457	0.0001071	0.0000001733	Zero.	Zero.	{ Heated above redness and allowed slowly to cool in wood-ash. "Soft."
	25.68	2635	2636					
	39.78	3981	3985					
	49.34	4859	4862					
	62.65	6031	6030					
II.	17.90	0.001815	0.001807	0.0001038	0.0000001629	0.00000031	0.0000000104	{ Heated galvanically below redness, then suddenly cooled. "Soft."
	31.39	3104	3100					
	47.58	4567	4572					
	56.53	5328	5349					
	63.72	5972	5956					
IV.	20.25	0.001112	0.001095	0.00005811	0.0000002006	0.0000490	-0.0000000273	{ Do. to redness, &c. "Glass-hard."
	29.20	1522	1526					
	46.50	2283	2268					
	59.90	2752	2761					
	61.35	2810	2810					
V.	18.07	0.000878	0.000881	0.00005156	0.0000001568	0.0000555	0.0000000165	{ Do. to redness, &c. "Glass-hard."
	21.93	1054	1056					
	47.76	2108	2105					
	61.32	2576	2571					
	63.44	2635	2640					

VI.	15.75	0.000703	0.000700	0.00004660	0.0000001390	0.0000605	0.0000000343	{ Heated galvanically to brick redness, then suddenly cooled. "Glass-hard."
	31.84	1347	1343					
	43.51	1756	1765					
	45.06	1815	1818					
VII.	59.42	2283	2278					
	19.04	0.000761	0.000764	0.00003807	0.0000001529	0.0000641	0.0000000204	{ Do. to brick-redness, &c. "Glass-hard."
	34.08	1288	1288					
	53.64	1874	1867					
VIII.	63.40	2108	2112					
	68.30	2225	2225					
	18.42	0.000644	0.000658	0.00003807	0.0000001279	0.0000690	0.0000000454	{ Do. to yellow heat, &c. "Glass-hard."
	31.15	1054	1062					
IX.	45.00	1464	1454					
	62.10	1874	1871					
	65.40	1932	1943					
						0.0000070		{ Do. to yellow heat, &c. "Glass-hard."

* This determination was made at a later date. With reference to its accuracy, the remarks under C and D apply.

TABLE III.—Rods 2.65 millims. thick, tempered by the mechanical artist Mr. Barth, of this city.

No.	τ .	E_{τ} , observed.	E_{τ} , calculated.	α .	β .	$a=T. E. H.$	b .	Remarks.
D.	32.28	0.002928	0.002918	0.00009447	0.0000001257	0.00001263	0.0000000476	Soft.
	45.55	4040	4042					
	46.88	4157	4153					
	55.54	4860	4859					
	56.24	4918	4915					
C.	20.32	0.001639	0.001628	0.00008265	0.0000001229	0.00002445	0.0000000504	Tempered "blue."
	31.98	2518	2517					
	32.90	2576	2586					
	47.93	3689	3678					
	49.80	3806	3811					
B.	32.40	0.001991	0.002004	0.0000646	0.0000000864	0.0000424	0.0000000869	Tempered "dark yellow."
	33.08	2049	2044					
	50.47	3045	3043					
	61.00	3630	3623					
	62.25	3689	3690					
A*.	10.86	-0.000117	-0.000121	-0.000000999	0.0000001068	0.00011710	0.0000000665	"Glass-hard."
	27.20	351	351					
	43.75	644	642					
	58.12	937	942					
	60.37	995	993					

* This rod (as also those of the following section C) is electronegative with reference to copper.

The following Tables IV. and V. contain the specific resistances of the rods already cited in Tables II. and III. The data are given in terms of mercury. The column S contains the total specific resistances, ΔS_0 the corresponding excess of the latter over that of the normal rod I. Of the ratio $\frac{\Delta S_0}{T. E. H.}$ mention will be made hereafter.

Let us denote by a and b the resistances of the two parts of the first branch of the bridge on each side of the sliding contact, by K' and K'' the resistances of the corresponding parts (thick copper wire) of the second branch, into which the unknown resistances W and R are inserted; finally, by δ the standard of comparison above referred to, in terms of which W and R are to be determined. When the current in the galvanometer is zero, we shall have for a particular position of the commutator, if we introduce

(1) W and R alone

$$\frac{a}{b} = \frac{W + K'}{R + K''};$$

(2) W and R with δ on the right,

$$\frac{a'}{b'} = \frac{W + K'}{R + K'' + \delta};$$

(3) W and R with δ on the left,

$$\frac{a''}{b''} = \frac{W + K' + \delta}{R + K''}.$$

Three similar equations may also be derived for the other position of the commutator, as only W and R are interchanged. From these six equations we deduce:—

First position,

$$\frac{W}{\delta} + \frac{K'}{\delta} = \frac{1}{\frac{b'}{a'} - \frac{b}{a}},$$

$$\frac{R}{\delta} + \frac{K''}{\delta} = \frac{1}{\frac{a''}{b''} - \frac{a}{b}}.$$

Second position,

$$\frac{W}{\delta} + \frac{K''}{\delta} = \frac{1}{\frac{a'}{b'} - \frac{a}{b}},$$

$$\frac{R}{\delta} + \frac{K'}{\delta} = \frac{1}{\frac{b''}{a''} - \frac{b}{a}}.$$

$\frac{K'}{\delta}$ and $\frac{K''}{\delta}$ were determined in the same way previous to the experiments, and their values checked from time to time.

In comparing the resistances I...IX. and A...D, the following plan was observed:—

W : δ ...	I	IV	IV		VIII	VIII	I	} And in the same way with A...D.
R : δ ...	II	II	V		VII	IX	IX	

Each of the data is therefore derived as a mean of four determinations. Possible heterogeneity of the wire $a+b$ thus becomes less effective.

As an example, I will add results obtained for the rod C (resist. = 0.0051 Siem. U.):—

		Compared with A.	Compared with B.	Compared with D.
1st position	W : δ =	0.1648	0.1654	0.1649
2nd position	W : δ =	0.1630	0.1632	0.1639
	Mean	0.1639	0.1643	0.1644

For the rod I (resist. = 0.0542 S. U.) on different occasions, 1.735 and 1.741 were found for W : δ .

As will be seen, the principle stress was put on the relative values of the resistances. To facilitate orientation, however, the results were approximately reduced to Siem. units by determining the standard δ in that denomination.

Assuming the coefficient for temperature for steel to be the same as that for copper, the results obtained will be good for 0° C. directly, δ having been previously reduced. Although this is only approximately true, the influence of this difficulty on the relative values of the resistances will be but slight, as the temperature of the room remained nearly constant.

TABLE IV.

No.	Resistance.	S.	ΔS_0 .	$\frac{\Delta S_0}{T. E. H.}$
I.	0.05417	0.1361	<i>Nil.</i>	—
II.	0.04305	0.1400	0.0039	1260
*IV. ¹ ...	0.11130	0.2337	0.0976	1990
V.	0.11060	0.2483	0.1122	2020
VI.	0.07846	0.2592	0.1231	2030
VII.	0.08740	0.2648	0.1287	2010
VIII.	0.15330	0.2779	0.1418	2050
*IX.	0.09417	0.2810	0.1449	2070

¹ The resistances marked with an asterisk were determined by using the magneto-inductor as current-generator. In the others a Smee element was employed.

TABLE V.

No.	Resistance.	S.	ΔS_0 .	$\frac{\Delta S_0}{T. E. H.}$.
D	0 00209	0·1654	0·0293	2320
C	0·00512	0·2065	0·0704	2860
B	0·00595	0·2271	0·0910	2150
A	0·00908	0·3804	0·2443	2090

The experiments now following, under the heads C and D, have more a descriptive character than one of precise measurement; the results are therefore given with one decimal less. For the determination of T. E. H. the rods coming under these heads were compared indirectly with the rod VIII. (Table II.), for the T. E. H. of which the number 0·000069 (as above found) was assumed.

C. The T. E. H. and ΔS_0 for glass-hard rods; diameter = 2·30 millims. These were hardened by the aid of a blast-lamp. The flame of the latter was for this purpose directed horizontally and placed directly over a trough containing cold water. In this way the rod could be transferred with great rapidity out of the flame into the water.

TABLE VI.

No.	T. E. H.	Resist- ance.	S.	ΔS_0 .	$\frac{\Delta S_0}{T. E. H.}$.	Remarks.
*[I]	0·000138	0·0121	0·421	0·285	2100	{ Heated yellow and sud- denly cooled. " " " " " " " " "
*[II]	131	—	—	—	—	
*[III]	130	—	—	—	—	
*[IV]	130	0·0129	0·442	0·306	2300	
*[V]	116	107	0·366	0·230	2000	{ Heated red and suddenly cooled. Obtained by reheating [III] and [IV] to yel- lowness and suddenly cooling.
*[V]'	136	123	0·430	0·294	2200	
*[III]'	133	—	—	—	—	

D. The T. E. H. and ΔS_0 of rods 0·678 in diameter, hardened † in the apparatus § II., afterwards annealed by immersion in hot linseed-oil.

† The T. E. H. of these rods in the glass-hard condition varied from 50 : 10⁶ to 60 : 10⁶.

TABLE VII.

No.	T. E. H.	Resist- ance.	S.	ΔS_0 .	$\frac{\Delta S_0}{T. E. H.}$	Remarks.
* 1.	0.000010	0.0540	0.157	0.021	2100	Annealed at 300°†.
* 2.	13	0616	158	022	1700	
* 3.	11	0537	159	023	2100	
* 4.	14	0691	163	027	1900	
* 5.	12	0592	166	030	2500	
* 6.	18	0763	177	041	2300	Annealed at 280°.
* 7.	24	0659	185	049	2000	
* 8.	33	0746	205	069	2100	Annealed at 240°.
* 9.	31	0807	206	070	2200	
*10.	36	0731	209	073	2000	Annealed at 200°.
*11.	57	0.1088	250	114	2000	Annealed at 150°.
*12.	58	0.1155	261	125	2200	
Soft iron. }	-0.000004	0.0420	0.133	-0.003	—	Heated in flame of a Bunsen burner to redness and allowed to cool in air.

E. The resistances thus far determined being so very small, it was to be feared that, in spite of the continuity apparent in their variation, errors from insufficient contact might have conspired in producing illusory results. For these reasons check-experiments with longer wires were made, the resistance of some of which (I. to V.) is sufficiently great to admit of direct comparison with the Siemens unit (étalon). To ensure a more homogeneous hardening, these wires were spirally wrapt around a round stick of wood, and the length and diameter of the coils resulting so determined that the whole during the process of heating could be brought within the mantle of the blast-flame. In other respects the method given under C was pursued.

TABLE VIII.

No.	Resist- ance.	Length.	Dia- meter.	S.	ΔS_0 .	Remarks.
I.	0.643	m 0.880	0.67	0.260	0.124	Heated yellow and immersed.
II.	379	917	96	296	160	" " " "
III.	245	928	1.19	293	157	" " " "
IV.	121	932	1.83	342	206	" br. red " "
V.	105	556	1.62	391	255	" br. yellow,, "
*VI.	0.0169	0.1733	2.15	354	218	" br. red " " [darker.
*VII.	0.0155	0.1732	2.15	325	189	Heated br. red and immersed; ends
*VIII.	0.0169	0.1740	2.15	352	216	" " " "

† Rods 1 to 5 remained in the bath during the whole process; the

Spirals II. and IV. were afterwards softened by heating to redness in a Bunsen burner. Their specific resistance in this condition was found to be (without allowing for the smaller section)

$$\text{*II. } S_0 = 0.154, \quad \text{*IV. } S_0 = 0.159.$$

Finally, in order to compare the results determined with induction-currents with those in which a Smee was employed, certain of the experiments were repeated. The agreement was entirely satisfactory.

DEDUCTIONS AND SUPPLEMENTARY EXPERIMENTS.

VII. *Hardness and Thermoelectric Properties of Steel.*

a. From the data contained in Tables II., III., and VII. we derive that *the thermoelectric position of steel progresses continuously with its degree of hardness*, or, in other words, thermoelectric and mechanical hardness are *direct* functions one of another.

This statement involves the assumption that a rod cannot pass from the glass-hard (maximum) to the soft state (minimum) without passing through every intermediate stage—or that, by proper methods of annealing, every state between the maximum and minimum could be produced. This, I dare say, will generally be admitted.

As of further interest I may add:—(1) that rods cut from the same wire and glass-hardened in the same way possess also the same thermoelectric hardness (Table VI.); (2) this is the case even when the rods are carefully rehardened (rods [III]', [V]', Table VI.); (3) that if we start from like maxima, the *thermo-current always passes from the less to the more annealed through warm*. (The direction of the current was independently observed.)

b. From an examination of the data obtained from different material, we infer that *the T. E. H. of soft and similarly annealed rods approximates to the same value**; that the value of

others were immersed but for two or three minutes. The temperature at which the rods 6 and 7 were annealed could not be determined with certainty, a microscopic air-bubble in the neck of the mercury-reservoir having given rise to a rupture of the thread.

* In order to compare the degree of hardness corresponding to a particular oxide tint with that corresponding to a particular temperature of the oil-bath, use was made of the Tables to be found in Frick's *Physikal. Technik*, 3 ed. p. 377; also Wagner, *Chem. Tech.* 8 ed. p. 29. On the authority of these, 230° corresponds to yellow-, 290° to blue-annealed. We should therefore expect rods 6, 7, and 8, 9 (Table VII.) to agree with rods C and B (Table III.), respectively. This is sufficiently the case.

this constant for glass-hard rods is remarkably different. The rods in Table VI., for instance, possess a T. E. H. amounting nearly to $140 : 10^6$; whereas in the rods in Table II. the maximum* value found does not exceed $70 : 10^6$. This may be due to a difference in thickness, or, more probably, to a difference in the composition of the rods examined.

These phenomena were further studied through the following experiments.

1. Commercial rods of different diameters were glasshardened and examined with reference to the current produced when one end of a couple was cooled with a wedge-shaped piece of ice. In general, a maximum of T. E. H. was observed in rods whose diameters lay between 1 and 2 millims. These experiments, however, are unsatisfactory, inasmuch as the composition of the rods enters as an element of disturbance which cannot be allowed for. For this reason experiments were made on thick bars, the parts of which had been filed to different diameters.

2. The halves of each of two pieces cut from the same rod 5 millims. in diameter were reduced† by filing to thicknesses of 3 and 1 millim. respectively, and glasshardened. During the process of heating, care was taken to raise all parts of the bars to the same degree of redness. On connecting the ends with the galvanometer and applying the ice wedge at the middle (where the diameter enlarged), very decided currents were observed passing from thin to thick through warm. Hereupon two cones were filed from the same material (5 millims. base and 50 millims. long). Point and base of the cones (previously glasshardened) being connected with the galvanoscope, the ice wedge applied at any point produced in each case currents from apex to base through warm, thus harmonizing with the previous experiments. Near the points only the results became uncertain. On bringing together the cones with the rod [IV] (Table VI.), the points were found thermoelectrically harder, the bases softer, than the former. On the other hand, the point of a fine needle prepared from the same material gave contrary indications; the fine point therefore was apparently softer than rod [IV].

3. Finally, two very gradually tapering cones were prepared from another steel rod 2.8 millims. in diameter. Con-

* These rods (Table II.), even when heated to the utmost white and suddenly cooled, remained strongly electropositive towards copper. T. E. H. therefore even in this extreme case was much less than $107 : 10^6$.

† It is to be observed that the thinner parts of these pieces sooner arrive at red heat and remain longer in this condition than the thicker parts. This applies equally to the cones.

nexions with the galvanometer being made, the application of the ice wedge to the parts near the base generated a current from thin to thick through warm, to parts near the apex a current in the opposite direction; finally, between these a position was found at which the application of the ice wedge produced no current at all. This occurred at parts about 1.5 millim. in diameter. The other cone gave like results.

4. When the bases of two similar cones of the same material are connected with the galvanometer and their apices brought into contact, upon warming the latter, a current in one direction or another will be produced—this from the fact that the points are rarely equally hard. Experiment shows, however, that by consecutively warming parts which lie symmetrically to the right and left of the apices in contact, currents in opposite directions are the effect. Herefrom it follows that these currents originate each in a single cone.

In endeavouring to generalize from these experiments, attention must be paid to the following points:—*a.* The maximum value of T. E. H. attainable is dependent on the quantity of carbon contained in the steel. The thermoelectric difference between rods of soft and suddenly cooled wrought iron can, for instance, be neglected in comparison with the corresponding difference of soft and hard steel. *b.* T. E. H. is influenced by the temperature of the rod when suddenly chilled (VII. *c*) as well as by the time of heating, the latter affecting the composition. *c.* By the form of the piece of steel and the method of sudden cooling, the internal structure of the mass being thereby modified (VII. *d*). *d.* Finally, we might add that in the time elapsing between the removal of the rod out of the fire and the subsequent immersion, the loss of heat by radiation will be relatively greater in the case of thin than in the case of thick rods.

With these facts in mind, we may conclude that the maximum values of T. E. H. attainable by glass-hardening rods of the same composition increases as the thickness diminishes; that as this dimension continues to decrease a diameter is reached at which the negative effects of decarbonization are equal to and finally overcome the positive effects due to diminution of diameter.

c. From the results contained in Table I. there follows: *the hardness of steel does not increase continuously with its temperature at the moment of sudden cooling, but at a point lying in dark-red heat the glass-hard state is suddenly attained.* From this point on, however, the degree of glasshardness (measured thermoelectrically) continues to increase with the temperature. This observation conduces to the conclusion

that the change of state due to glasshardening is chemical in its nature. In this opinion I believe most chemists at present also concur, it being assumed that the uncombined carbon in soft steel is, by sudden immersion, converted into the chemically combined. In summing up the facts by which the hypothesis is furthermore supported, we will mention, in the first place, the detailed analogy* which exists between the white pig used in the manufacture of Bessemer steel and glass-hard steel, on the one hand, and ordinary (grey) cast iron and soft steel on the other, the former containing carbon only in the combined, the latter also in considerable proportion in the uncombined state. Secondly, hardening by a process of wire-pulling, hammering, &c., will in all probability increase the specific gravity of steel; hardening by sudden cooling, as is well known, diminishes it. In the former case, the thermoelectric current usually passes from soft to hard through warm†; in the latter, in a contrary direction. In drawn wire the specific resistance is smaller‡ in hard-tempered, and greater than that of the same wire in the soft state.

Considering these facts as a whole, we are perhaps justified in distinguishing between a process of *chemical* and a process of *mechanical* hardening. This, however, does not prevent us from paying due regard to the series of physical phenomena which always accompany the former. To these the peculiar internal structure of glass-hard bars, the warping which frequently attends sudden cooling &c., are to be referred. We conclude, therefore, that the cause chiefly influential in bringing about glasshardness in steel is chemical in its nature, and that in consequence of the physical phenomena which invariably accompany it the degree of glasshardness is more or less modified. On the latter ground the continual increase of the T. E. H. after the critical temperature above referred to has been reached is to be explained.

d. The observed T. E. H. can of course only be assumed as directly expressive of its hardness when the rod under experimentation is homogeneous throughout. This is approximately the case for thin rods. In thick bars which in the glass-hard state may be considered as made up of a series of concentric cylindrical shells, the hardness of which decreases rapidly as

* See Wagner's *Chem. Technol.* 8 ed. pp. 14, 15, 29. These analogies refer to mechanical properties, method of preparation, colour.

† Magnus, *Pogg. Ann.* lxxxiii. p. 469, 1851; Sir W. Thomson, *Phil. Trans.* iii. p. 722, 1856.

‡ Mousson, *N. Denkschr. der schweiz. Gesellsch.* xiv. 8. pp. 1-90. The reader is also referred to Chwolson, *Carls. Repert.* xiv. p. 15.

we pass from the exterior to the interior, the circumstances become more complicated.

Furthermore, suppose the ends of a thick glass-hard cylinder to be kept at temperatures T and t ($T > t$). In this case, since each of the infinitesimally thin cylindrical shells has a particular T. E. H. corresponding to its hardness, we are led to infer that thermo-currents closing themselves in the interior of the cylinder are the result—the direction of these in the outer (harder) parts being from t to T , in the inner from T to t . In fig. 3 (vertical section) the hypothetical condition of the cylinder in this case is indicated. As will be seen, its electrical state corresponds with that of a rod circularly magnetized.

For the purpose of studying this question experimentally, a steel cylinder, 30 millims. in diameter and 50 millims. long, was turned and glasshardened. This was placed vertically directly before the needle of a magnetometer (the deflection of which could be read off with telescope and scale) in such a manner that the position of equilibrium of the needle was left unaltered. The relative position of needle and cylinder, in other words, was such that the axis of the former, if prolonged, would intersect the axis of the latter at its middle point. Upon now cooling the upper end of the cylinder with a piece of ice, or warming by projecting a jet of steam against it, very decided deflections were observed toward the right or left respectively, which increased with the difference of temperature $T - t$, and vanished as this difference became *nil*.

As the cylinder was not magnetic, it is improbable that these phenomena can be referred to a change in the state of magnetic distribution. With reference to the direction of the currents, however, no simple results could be arrived at.

e. In § VII. *c*, we ascribed the very high value of the T. E. H. of a glass-hard steel rod to the large proportion of chemically combined carbon contained therein. If this be true, the thermoelectric difference between soft steel and soft iron, in both of which combined carbon is either wholly absent or exists only in traces, must be quite small. This inference is supported by the data actually found for soft iron. Making allowance for the difference of circumstances involved, the result to be derived from experiments of Kohlrausch and Ammann also agrees sufficiently herewith.

On the other hand, Joule* has long since shown that ordinary cast iron is thermoelectrically negative towards copper, all the more, therefore, towards soft steel—a result which we should be inclined to predict from the quantity of combined carbon contained.

* Joule, *Phil. Mag.* [4], vol. xv. pp. 538, 539, 1857.

The minimum values of T. E. H. (obtained by cooling the red-hot bar as slowly as possible) of different kinds of steel* and of soft iron are therefore approximately the same; whereas the maximum value of this constant (obtained by cooling the highly heated bar as rapidly as possible) differ enormously, this difference being a direct function of the quantity of carbon contained.

f. Sir W. Thomson† has shown that in a thermoelement consisting of magnetized and unmagnetized steel of the same hardness and form, thermo-currents due only to magnetic difference can be generated. The direction of these currents was found to be in one case from unmagnetized to longitudinally magnetized through warm, in another from transversely magnetized to unmagnetized through warm, therefore also from transversely to longitudinally magnetized through warm.

For the purpose of informing myself of the magnitude of the thermo-difference thus produced, the following experiment was made:—A soft rod (I., Table II.) was tested for its electromotive force when combined with copper (as described above, p. 348), and the locus of the equation $E_r = \alpha\tau - \beta\tau^2$ constructed from fifteen very carefully made observations.

Hereupon a large magnetic battery weighing 40 lbs. was so placed that each of the ends of the horseshoe touched a receiver. The distance between the poles of the magnet and the corresponding ends of the steel rod was thus about 5 centims. A second series of fifteen observations was now made. Upon comparing the locus of the latter with that of the former, the two curves coincided so completely that no influence could be discerned. Herefrom we conclude that the thermoelectric effect due to a difference of magnetic state may, in comparison with those which can be produced by a difference in hardness, be completely neglected.

g. A very curious analogy was found in comparing the results at which Fromme‡ arrived, in studying the specific gravity of differently tempered steel, with the T. E. H. of similar rods as found in my experiments. Dr. Fromme, if I infer correctly, limited his experiments to rods whose diameter was greater than 2 millims. and less than 7 millims. His results are contained in the following Table, the volume of the soft bar being put = unity.

* Steel is here used as distinguished from iron only by containing a greater proportion of carbon. No attention has been paid in this paper to the effects of P, S, Si, Mn, &c., so often present in both.

† Phil. Trans. iii. pp. 722-727, 1856.

‡ Fromme, *Götting. Nachr.* 165, 1876.

TABLE IX.

Remarks.	Volume.	Increase of same.	T. E. H.
Soft	1·000	0·000	0·000000
"Blue"	1·002	0·002	0·000024
"Yellow"	1·005	0·005	0·000042
"Glass-hard"	1·010	0·010	0·000117

In the fourth column the T. E. H. of rods cited in Table III. (these, as I believe, corresponding very nearly to those for which the data of Fromme apply) is added. If we take into consideration that the results were obtained from different material, *the parallelism observable is striking*. To the very large difference between glass-hard and yellow-annealed, when compared with the much smaller difference between yellow and blue, blue and soft, as seen in both observations, I would once more call attention. The fact that glass-hard rods can be considerably annealed at comparatively low temperatures, must be regarded as an adequate indication of their unnatural strained condition.

A second result of Fromme, that the specific gravity of thin rods suffers *greater* loss by glasshardening than that of thick rods, also harmonizes with the conclusions drawn in § VII., *b*, with reference to the T. E. H. of such bars.

VIII. Hardness and Specific Resistance of Steel.

From the data found for specific resistance of steel rods of different hardness, inferences analogous to the above may be deduced:—

a. The specific resistance of steel increases continuously with its mechanical hardness.

b. Rods like-annealed differ but slightly, glass-hard rods considerably with respect to their specific resistance.

c. On comparing the values found for the ratio $\frac{\Delta S_0}{T. E. H.}$, we infer that the specific resistance of steel is approximately a linear function of its thermoelctric hardness. In fig. 5 these results are graphically represented.

I will remark, however, that the assumption of proportionality as based on the above figures is to be regarded as a first approximation only, notwithstanding the fact that the discrepancies fall within the errors of experiment. A rigid discussion of the latter comes more appropriately within the scope of another paper, soon to appear. In this place I would

only call attention to the following. In the thick bars experimented upon the value of $\frac{\Delta S_0}{T. E. H.}$ is usually too large—a fact

which is easily accounted for, as the unavoidable resistances of contact above referred to will in this case have a relatively great effect, the resistance of the bars themselves being very small. In the rods included in Table VI. it was impossible to secure a uniform redness throughout, the ends invariably remaining darker. As, however, T. E. H. depends principally on the warm end, and ΔS_0 on the mean hardness of the whole bar, we have thus a second cause for an overlarge ratio.

So much, however, I think I have fully established, that T. E. H. and specific resistance of steel throughout their variation are very simple functions one of another. T. E. H. and specific resistance must therefore be looked upon as effects of the same cause, as phenomena having some very intimate connexion.

d. Particular attention must here be called to the remarkable result that the specific resistance of steel can by a process of glasshardening be increased to *nearly three times* its value in soft steel*. As this datum far exceeds that determined by Mousson (about 25 per cent.), it is not without some hesitation that I make it public. The care bestowed on the experiments, however, together with the regularity observable in the variation of the results, I believe, sufficiently ensure their correctness. See moreover § VI., E†.

e. As deserving special notice, I will further add that the thermo-current always passes from the bar with *greater* to the bar with *less* specific resistance. The few exceptions to this fact in the Tables were afterwards found to be referable to errors of experiment by direct observation‡.

* It is to be observed, however, that the difference between the specific resistance of steel in the soft and hard states is dependent on the composition, increasing with the quantity of carbon contained from a very small value in soft iron to the very large value above announced for steel.

† Chwolson reports the increase of resistance due to glasshardening to be only 0.6 per cent. This I can only explain by supposing the results of this observer to have been obtained from wires suddenly cooled at a temperature below that referred to in VII., c.

‡ I would here again refer to the fact that, according to Magnus, Thomson, and Mousson, drawn steel wire and hard-tempered steel are on different sides of soft steel, both with respect to their thermoelectric properties and their specific resistance. Thomson furthermore finds transversely magnetized steel electronegative towards soft steel, this again towards longitudinally magnetized steel; Auerbach (Wiedemann's *Ann.* v. p. 316, 1878), analogously, that the specific resistance of hard steel con-

f. Like the T. E. H., so also the specific resistance of steel approximates to the value of this constant for soft iron. Upon the value found for the ratio $\frac{\Delta S_0}{\text{T. E. H.}}$, however, not much reliance can be placed, the factors involved being too small to admit of accurate determination.

With cast iron no experiments were made.

IX. *Remarks on the above, considered as auxiliary to the determination of the relation between hardness and magnetic moment.*

In this place I will avail myself of the experiments of Ruths on the relation between hardness and magnetic moment, these being perhaps the most comprehensive. With sufficient approximation for our purpose, we will put the T. E. H. of glass-hard rods = $120 : 10^6$, that of the yellow annealed = $40 : 10^6$, of the blue = $20 : 10^6$; and with these as abscissæ and Ruth's values for the corresponding permanent magnetic moments ("in millions of absolute units," mg.-mm.-sec. syst.) as ordinates, suppose the curves belonging to each of the bars † to be constructed. These curves are given in fig. 6, the attached number referring to the ratio between the length and diameter of each rod.

I will omit the interesting deductions which Ruths makes from his data, these going beyond our present purpose.

From an inspection of the curves we derive the following important result,—viz. that, like the electrical properties of steel and its specific gravity, so also the maximum of permanent magnetism is largely modified by the different degrees of glasshardness of the bar (*i. e.* those lying above yellow-annealed and scarcely distinguishable mechanically). In the second place, the results of Ruths, being obtained from comparatively thick bars, are largely influenced by structure. In view of these facts, I deem the hope by no means too sanguine, that, if to avoid complications from structure we experiment on thin rods, the maximum of permanent magnetism may be empirically expressed in terms of the dimensions and T. E. H. only—that, furthermore, from the parallelism discovered in

tinuously increases as the rod passes from a condition of saturated longitudinal to a condition of saturated circular magnetization.

The result above enunciated has therefore probably even a more general signification.

† The data employed are those obtained by Ruths for rods 120 millims. long and 1.7, 2.4, 2.9, 3.8, 4.9, 5.9 millims. respectively in diameter, with very powerful magnetizing forces.

the variation of specific gravity and the electric properties of steel, the T. E. H., specific resistance, and magnetic moment are in some very intimate way connected with the volume of a unit of mass. This would imply a connexion of these phenomena with the intermolecular spaces of steel.

In conclusion, it gives me great pleasure to acknowledge my indebtedness to Prof. Kohlrausch for much kind assistance throughout the course of the experiments.

Phys. Inst. Univ. Würzburg,
February 1879.

XLI. *Influence of Atomic Weight.* By THOMAS CARNELLEY, D.Sc., Assistant Lecturer on Chemistry in the Owens College.

[Continued from p. 324.]

Influence of Atomic Weight on the Physical Properties of Compounds.

SO far, we have endeavoured to show the influence of atomic weight on the properties of the elements; we shall now continue this subject with regard to the influence of atomic weight on the physical properties of compounds.

Mendeljeff's Law of Periodicity runs thus:—"The properties of the elements are a periodic function of their atomic weights." This law may be supplemented as follows:—"The properties of the compounds of the elements are a periodic function of the atomic weights of their constituent elements." In a paper recently (June 19, 1879) read before the Royal Society the author has shown that this holds good as regards the melting- and boiling-points and heats of formation of the normal halogen compounds of the elements and of certain compounds of the elements with monatomic alcohol radicals; and for the present we shall limit ourselves to these.

A. Melting- and Boiling-points and Heats of Formation.

I. *Normal Chlorides, Bromides, and Iodides of the Elements.*—In whatever way the melting-points, boiling-points, and heats of formation of these compounds be arranged, provided only they are arranged systematically, we always find that certain definite and regular relations may be traced between the numerical values for the above-mentioned physical properties and the atomic weights of the compounds. Some of the more important conclusions arrived at are as follows:—

(1) *The melting- and boiling-points and heats of formation of*

the normal halogen compounds of the elements are a periodic function of the atomic weights of the constituent elements. For if the elements be arranged in the order of their atomic weights, then the melting-points, boiling-points, and heats of formation of their halogen compounds rise and fall periodically nine times, these periods corresponding exactly with Mendeljeff's nine series of elements. The maxima occur at the positive and the minima at the negative end of each series.

(2) *The influence of the halogen on these same physical properties increases with the number of its atoms in the compound, thus :—*

Boiling-point.	Boiling-point.	Boiling-point.
I Br = 390*	Sb Br ₃ = 549	Sn Br ₄ = 474
I Cl = 373	Sb Cl ₃ = 496	Sn Cl ₄ = 388
<u>17</u>	<u>53</u>	<u>86</u>

(3) *In any normal halogen compound the influence of either of the elements on the melting- or boiling-point increases with its own atomic weight, and decreases with the atomic weight of the other element. Thus :—*

Melting-point.	Melting-point.
Sb Br ₃ = 363	Sb I ₃ = 438
As Br ₃ = 295	Sb Br ₃ = 363
<u>68</u>	<u>75</u>

In each of the above pairs of compounds which contain one element in common the melting-point increases with the atomic weight of the variable element, and that whether the latter be the positive or negative constituent—thus showing that the influence of an element on the melting-point of one of its compounds increases with its own atomic weight. That it decreases with the atomic weight of the other element is shown by the following examples :—

Melting-point.	Melting-point.
As I ₃ = 419	Sb I ₃ = 438
As Br ₃ = 295	Sb Br ₃ = 363
<u>124</u>	<u>75</u>

Here the substitution of I for Br produces a larger increase in the melting-point in the case of the As than in that of the

* These numbers are all reckoned from the absolute zero -273°C. , and this is the case with all melting-points and boiling-points subsequently referred to.

Sb compounds, the atomic weight of As being less than that of Sb. Again

Melting-point.		Melting-point.	
Sn Br ₄	= 303	Sn I ₄	= 419
Si Br ₄	= 260	Si I ₄	= 393
	<u>43</u>		<u>26</u>

As before, the substitution of Sn for Si produces a greater influence on the melting-point in the case of the bromides than in that of the iodides, *i. e.* where the atomic weight of the negative element is least.

(4) *The melting- or boiling-point or heat of formation of a bromide is always nearer to that of the corresponding chloride than to that of the corresponding iodide; and the melting- or boiling-points of the halogen compounds of the middle member of three consecutive elements of the same group are always nearer to those of the first (i. e. the one with least atomic weight) than to those of the last member. Thus:—*

Melting-point	=	SbCl ₃ . 345	SbBr ₃ . 363	SbI ₃ . 438
		Difference = 18		75
Melting-point	=	NaCl. 1045	NaBr. 981	NaI. 901
		Difference = 64		80
Boiling-point	=	PCl ₃ . 351	AsCl ₃ . 405	SbCl ₃ . 496
		Difference = 54		91

The former of these phenomena probably depends on the fact that the atomic weight of Br is nearer to that of Cl than to that of I; and the latter on the fact that the atomic weight of the middle member of three consecutive elements of the same group is always less than the mean of those of the other two elements; thus—

Atomic weight	=	Cl. 35·5	Br. 80	I. 127
		Difference = 44·5		47
Atomic weight	=	P. 31	As. 75	Sb. 122
		Difference = 44		47

We have here, therefore, a good instance of the influence of atomic weight on the physical properties of compounds.

(5) The melting- and boiling-points of the halogen compounds of the elements belonging to the first and second groups of Mendeljeff's classification are widely separated from those of the other groups, being in fact considerably higher. Different relations, too, often appear to exist between the melting-points of *even* members of these two groups from those which exist between groups (3-7) inclusive; while the compounds of the elements which are often placed in the *odd* divisions of the first and second groups are generally altogether irregular. In the case of the odd members of the first group, this may be explained by the fact that it is very uncertain whether Cu, Ag, and Au really belong to the same group as Na or not, as pointed out by Mendeljeff in his memoir on the Periodic Law; for he places these elements not in the first group along with Na, but in the eighth with Fe, Pd, Pt, &c.

In the paper above referred to it has been shown how the preceding relations may be applied

(1) *To the calculation of unknown melting- and boiling-points.*—The following are instances of melting-points predicted by this method having been verified by experiment:—

	Predicted.	Experiment.
CsCl.....	Below 959	904 } *
CuBr	782	777 } *
BeCl ₂	(820-870)	(858-890)
BeBr ₂	(802-820)	(858-890)

(2) *To the determination of the atomic weights of elements* when the application of the methods of specific heat and vapour-density are inadmissible or uncertain. By this means the atomic weight of Be has been found to be 9.2, thus agreeing with the determination of the specific heat made by Emmerson Reynolds, and proving Nilsson and Pettersen's determination to be incorrect; for they found a value for the specific heat corresponding to the atomic weight 13.8.

As far as existing data allow us to judge, the compounds of the elements with fluorine and also with monatomic organic radicals, likewise obey the same laws as those of the halogen compounds. The fluorides, however, cannot be strictly compared with the chlorides, bromides, and iodides, since F is an *even* member of the seventh group; whereas Cl, Br, and I are *odd* members.

Boiling-points of Organic Compounds in general.—The boiling-points and melting-points of carbon compounds depend:—(1) On the atomic weights or nature of the consti-

* These two numbers have not been previously published.

tuent elements ; (2) On the position or arrangement of these elements in the molecule. The first of these is more especially rendered evident in homologous series, and the second in isomeric compounds. Of the latter, however, we shall have little or nothing to say in the present communication, as it is intended, if opportunity admits, to refer to it at length in a subsequent paper.

The connexion between the boiling-points and composition of organic compounds as pointed out by Kopp (*Ann. Chem. Pharm.* xvi. pp. 2, 230, xcviii. pp. 267, 367) is so well known that it will be unnecessary to go into detail with regard to it. It will be sufficient to mention his more important conclusions :—(1) Analogous compounds presenting the same difference of composition very frequently differ by the same amount in their boiling-points. A compound containing $x\text{C}$ more or less than another compound of analogous function generally boils at a temperature $29x$ degrees higher or lower than the latter ; and if it contains $x\text{H}$ more or less, it generally boils at $5x$ degrees lower or higher. These rules are best applied in the case of compounds belonging to the same homologous series. In the fatty acids $\text{C}_n\text{H}_{2n}\text{O}_2$ and corresponding alcohols and compound ethers each addition of CH_2 raises the boiling-point, on an average, by $(29 - 2 \times 5) = 19^\circ$, thus agreeing with the above rule. (2) An acid $\text{C}_n\text{H}_{2n}\text{O}_2$ boils 40° above the corresponding alcohol $\text{C}_n\text{H}_{2n+2}\text{O}$. (3) A compound ether $\text{C}_n\text{H}_{2n}\text{O}_2$ boils 82° below the acid isomeric with it. In other series of compounds the difference in boiling-point corresponding to a difference of CH_2 is mostly regular ; but it is sometimes more and sometimes less than 19. As a rule, the greater the quantity of O in a compound, the smaller is the effect on the boiling-point of an increase of CH_2 in the composition. In the halogen compounds of the alcohol radicals $\text{C}_n\text{H}_{2n+1}$, a difference of CH_2 corresponds to a difference of 24 to 31 degrees in the boiling-point. As an instance of boiling-points calculated in this way being subsequently verified by experiment, I give the following :—

	Calculated.	Found.
Heptyl chloride	158	159.2
„ bromide	179	178.5
„ iodide	202	201.0
„ acetate	189	191.5

Notwithstanding, however, the near agreement of many of the calculated with the experimental boiling-points, Linne-
mann (*Ann. Ch. Pharm.* clxii. p. 39), who has carefully de-

* C. F. Cross (*Chem. Soc. Journ.* 1877, ii. p. 128).

terminated the boiling-points of a large number of compounds, concludes that the differences of boiling-point between consecutive members of homologous series are by no means exactly equal, but exhibit considerable variation, even as much as 3° .

The following are a few further examples of regularity in the boiling-points of organic compounds which have recently been pointed out.

In silicon compounds the substitution of (C_2H_5) for Cl raises the boiling-point, as does also an increase in the number of oxygen atoms (Ladenburg, *Ann. Ch. Pharm.* clxiv. p. 300):—

Boiling-point. °	Boiling-point. °	Boiling-point. °
$SiCl_4$... 58		
$SiCl_3(C_2H_5)$... 100	$SiCl_3(C_2H_5)O$... 104	
$SiCl_2(C_2H_5)_2$... 129	$SiCl_2(C_2H_5)_2O$... ?	$SiCl_2(C_2H_5)_2O_2$... 137
$SiCl(C_2H_5)_3$... 144	$SiCl(C_2H_5)_3O$... 147	$SiCl(C_2H_5)_3O_2$... 151
$Si(C_2H_5)_4$... 152	$Si(C_2H_5)_4O$... 153	$Si(C_2H_5)_4O_2$... 156
	Boiling-point.	Boiling-point.
$SiCl(C_2H_5)_3O_3$... 156		
$Si(C_2H_5)_4O_3$... 159		$Si(C_2H_5)_4O_4$... 166

Here the differences between every two consecutive members of each series diminish as we pass down each vertical column, *i. e.* as the number of chlorine atoms replaced by C_2H_5 increases; and these same differences also diminish in each horizontal line from left to right, *i. e.* as the number of O atoms increases. The differences between each two consecutive members of a horizontal series increase from left to right, *i. e.* with the number of O atoms.

Mendeljeff has called attention to the fact that Si compounds boil lower than the corresponding C compounds.

The iodides of the alcohol-radicals always boil 53° higher than the corresponding amides (Linnemann, *Ann. Ch. Pharm.* clxii. p. 12), thus:—

	Ethyl.	Propyl.	Butyl.	Isobutyl.	Tertiary butyl.	Amyl.
I = ...	72°	102·2	129·9	120·6	98·5	148·0
NH ₂ = ...	18·7	49·0	76·5	67·5	46·0	95·0
Difference	53·3	53·2	53·4	53·1	52·5	53·0

Salomon (*Journ. f. Chem.* [2] vi. p. 433) has shown that
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in the case of the ethyl carbonates and ethyl sulphocarbonates, the introduction of an atom of S into the ethylated radical is accompanied by a rise of 40° in the boiling-point, and by a rise of 44° when the S is introduced into the carbonyl group, except as regards the first member of the series:—

$\text{CO} \left\{ \begin{array}{l} \text{OC}_2\text{H}_5 \\ \text{OC}_2\text{H}_5 \end{array} \right.$	$\text{CO} \left\{ \begin{array}{l} \text{OC}_2\text{H}_5 \\ \text{SC}_2\text{H}_5 \end{array} \right.$	$\text{CS} \left\{ \begin{array}{l} \text{OC}_2\text{H}_5 \\ \text{OC}_2\text{H}_5 \end{array} \right.$	$\text{CO} \left\{ \begin{array}{l} \text{SC}_2\text{H}_5 \\ \text{SC}_2\text{H}_5 \end{array} \right.$
Boiling-point... 125°	156°	161°	196°
	$\text{CS} \left\{ \begin{array}{l} \text{OC}_2\text{H}_5 \\ \text{SC}_2\text{H}_5 \end{array} \right.$	$\text{CS} \left\{ \begin{array}{l} \text{SC}_2\text{H}_5 \\ \text{SC}_2\text{H}_5 \end{array} \right.$	
	Boiling-point ... 200°	240° .	

From the following Table it is seen that an ethereal salt of a hydroxy-acid boils 20° higher than its methoxy derivative, and that the ethyl salts of a hydroxy- and an ethoxy-acid have nearly the same boiling-point. Further, the boiling-point of an ethereal salt rises 20° for the first addition of CH_2 to the alcohol-radical and 26° for the second; but only 6° and 8° respectively for a similar addition to the saline radical (Schreiner, *Ber. deut. chem. Ges.* xii. p. 179). Again, the methyl and ethyl acids boil exactly 40° higher than their corresponding ethyl salts.

Alcohol-radical.		Glycollates.				Lactates.		
		OH.	CH ₃ .	C ₂ H ₅ .	C ₃ H ₇ .	OH.	CH ₃ .	C ₂ H ₅ .
Saline radical.	{	OH ...	— ^o —	178 ^o	199 ^o	— ^o —	— ^o —	195 ^o
		CH ₃ ...	151	132	152	178	145	—
		C ₂ H ₅ ...	160	138	158	184	155	136
		C ₃ H ₇ ...	170	147	166	192	—	—

Alcohol-radical.		Oxybutyrates.			Oxyisobutyrate.		
		OH.	CH ₃ .	C ₂ H ₅ .	OH.	C ₂ H ₅ .	
Saline radical.	{	OH ...	— ^o —	— ^o —	— ^o —	212 ^o	— ^o —
		CH ₃ ...	—	—	—	—	—
		C ₂ H ₅ ...	167	148	167	151	155
		C ₃ H ₇ ...	—	—	—	—	—

Those benzene hydrocarbons containing an even number of methyl groups are solid, whereas those containing an odd number are liquid, or melt very much lower. The difference between the boiling-points also changes periodically from 30° to 25° (Jannasch, *Ann. Chem.* clxxvi. p. 283). These facts are probably due to the circumstance that in those hydrocarbons containing an even number of methyl groups the latter

are symmetrically arranged; whilst in those containing an odd number the arrangement is generally asymmetrical.

	Benzene, $C_6 H_6$.	Toluene, $C_6 H_5 CH_3$.	Paraxylene. $C_6 H_4 (CH_3)_2$.	Pseudo-cumene. $C_6 H_3 (CH_3)_3$.	Durene. $C_6 H_2 (CH_3)_4$.
Melting-point. } =	7°	Liquid.	15°	Liquid.	80°
Boiling-point. } =	81°	111°	136°	166°	191°

Groebe (*Ber. deut. chem. Ges.* vii. p. 1629) has shown that, as a general rule, diphenylene compounds boil 40° higher than the corresponding compounds of diphenyl, phenanthrene alone being an exception; thus:—

	Benzo-phenone, $C_6 H_5 \diagup CO.$ $C_6 H_5 \diagdown$	Diphenylene ketone, $C_6 H_4 \diagup CO.$ $C_6 H_4 \diagdown$	Diphenyl methane, $C_6 H_5 \diagup CH_2.$ $C_6 H_5 \diagdown$	Diphenylene methane, $C_6 H_4 \diagup CH_2.$ $C_6 H_4 \diagdown$
Boiling-point. } =	295°	336°	262°	302°

As regards the boiling-points of isomerides, Naumann (*Ber. deut. chem. Ges.* vii. pp. 173, 206) has pointed out that the simple chain formulæ allow of greater condensation of the molecule, and consequently give a higher boiling-point; whilst the more this form is disturbed by side chains, the lower the boiling-points. Also, the boiling-points of meta-merides of analogous constitution and containing O, are the lower the nearer the O is to the centre of the chain of atoms; thus:—

	Boiling-point.
Butyl alcohols.....	$\left\{ \begin{array}{l} CH_3 \cdot CH_2 CH_2 \cdot CH_2 \cdot OH \quad 116 \\ CH_3 \cdot CH_2 CHOH \cdot CH_3 \quad 99 \end{array} \right\}$
Methylpropyl ether...	$CH_3 \cdot CH_2 CH_2 \cdot O \cdot CH_3 \quad 51$
Diethyl ether	$CH_3 \cdot CH_2 \cdot O \cdot CH_2 \cdot CH_3 \quad 35$

The theory has been advanced by Burden (*Phil. Mag.* [4] xli. p. 528) that at the boiling-point (Bar.=760) the velocity of the molecules is a constant for all liquids, viz. =1140 feet per second. This number is obtained by the use of the equation

$$v \text{ (at b.-p.)} = \frac{273 + \text{b.-p.}}{273} \sqrt{\frac{(\text{velocity of H gas at } 0^\circ)^2}{\text{vapour-density of the liquid}}}$$

the velocity of H at 0° C. being =6050 feet per second. For the *raison d'être* of this equation, reference must be made to the original paper. Burden gives a large number of Tables

in support of his view. The following are the mean velocities calculated by him for several series of compounds:—

Paraffins.....	1138	feet per second.
Olefines	1155	” ”
Other hydrocarbons	1182	” ”
Aromatic hydrocarbons...	1243	” ”
Simple ethers.....	1131	” ”
Methylic salts	1185	” ”
Ethylic salts	1140	” ”
Other ethereal salts	1125	” ”
Anhydrides	1167	” ”

The alcohols and acids, however, give a much higher velocity (1300–1800 feet per second) at their boiling-points than the above. But it is probable that these compounds are not so exceptional as might at first sight appear; for the acids of the series $C_n H_{2n} O_2$ exhibit a remarkable variation in the volume-vapour which they furnish at different temperatures (Bineau). Thus, formic acid at its boiling-point (101°) has a vapour-density of 46 in place of 23, and gains its proper bulk only at 213° . A similar thing also applies to acetic acid. If, therefore, these facts be taken into consideration, formic acid will show at its boiling-point a velocity of 1160 feet, and acetic acid a velocity of 1134 feet per second. Herwig has shown that ethyl-alcohol also possesses an exceptional vapour-density at or near its boiling-point.

Pictet has recently pointed out (*Compt. Rend.* lxxxviii. pp. 855, 1315), as already stated, that there exists a simple relation between the atomic weight, coefficient of expansion, and melting-point of a solid body*. He also finds that a similar relation (*Compt. Rend.* lxxxviii. p. 1315) exists between the atomic weight, coefficient of expansion, and boiling-point of a liquid; thus:—

- (1) The length of oscillation of liquid molecules at the

$$\text{b.-p.} = l = \frac{\alpha}{\sqrt[3]{\frac{d}{p}}},$$

- (2) $lT = Kn$;

where α = mean coefficient of expansion between the melting- and boiling-points, d = density, k = constant, T = boiling-point reckoned from -273°C. , p = the physical molecular weight, *i. e.* those weights of different liquids which absorb equal quantities of heat on their temperature being raised from 0° to 1°C. ; and this, being inversely as the specific heat, is a

* See also a paper by Wiebe on this subject in the *Chemical News* for September 26, 1879, p. 154

simple multiple of the chemical or true molecular weight ; n is a number proportional to the number of liquid molecules in a given unit of length.

Melting-points of Carbon Compounds in general.—But little attention has been directed to the influence of atomic weight on and the relations between the melting-points of carbon compounds. It is, however, a subject which would no doubt repay careful investigation, and is especially important from the fact that this physical property is the one which renders the most service in the recognition of the solid compounds of carbon. The following relations will be of interest.

As pointed out above in speaking of boiling-points, those aromatic hydrocarbons containing an even number of methyl groups are solid, whilst those containing an odd number are liquid. A similar thing applies to the chlorine, and doubtless to other derivatives of benzene ; for those containing an odd number of Cl atoms melt much lower than those with an even number, thus :—

	C_6H_6 .	$C_6H_5 \cdot Cl$.	$C_6H_4 \cdot Cl_2$.	$C_6H_3 \cdot Cl_3$.	$C_6H_2 \cdot Cl_4$.	$C_6H \cdot Cl_5$.	C_6Cl_6 .
Melting-point. } =	3°	-40°	53°	17°	139°	74°	26°

The difference between those compounds containing an odd number of Cl atoms is constant, viz. 57°, and also the difference between those containing an even number of Cl atoms, viz. 86°. These facts, as previously intimated, probably depend on the symmetrical or asymmetrical arrangement of the Cl atoms.

As a general rule, the melting-points of a series of homologous compounds rise as we ascend in the series ; but there is one remarkable case known in which the reverse is the case, thus :—

	Melting-point.
Methyl terephthalate	140°
Ethyl	44
Propyl	31
Butyl	liquid
Isopropyl	56
Isobutyl	53

The relations between the melting-points of organic compounds and their chemical composition offer so wide a field of investigation, and one which has been so little touched, that it is my intention at an early date to trace these relations as completely as possible, especially as regards the influence of the position of the atoms, and more particularly with respect to the influence of symmetry. I have already worked a good

deal at this subject, and have come to the following conclusions:—

(1) The melting-points of those compounds the atoms of which are symmetrically arranged, are higher than in the case of compounds in which the atomic arrangement is asymmetrical. (2) The stability (and therefore the heat of formation) of symmetrical compounds is greater than that of asymmetrical compounds isomeric with them. If this be true, then it would follow that the heats of combustion of the former compounds are less than those of the latter.

The application of the first of these conclusions may be made, first, to isomeric compounds, and, secondly, to compounds belonging to the same homologous series; whilst the second refers of course only to isomeric compounds. I reserve, however, the details of this investigation for a future communication. Since working this subject out at some length, I have noticed that Mr. Henry Watts, F.R.S., in the last Supplement (vol. viii. p. 221) of his 'Dictionary of Chemistry' has shown in a very clear and decisive manner that "The more symmetrical the constitution of a benzene derivative, the greater is the resistance which it offers to the passage from the solid to the liquid state," or, in other words, the higher the melting-point.

Freezing-points of Saline Solutions.—De Coppet (*Ann. Chim. Phys.* [4] xxiii. p. 366, xxv. p. 502, xxvi. p. 98) has shown, as the result of a long series of experiments, that for bodies belonging to the same molecular group the coefficient of depression of the freezing-point is inversely as the molecular weight; i. e. the molecular depressions of the freezing-point are equal. A similar relation also exists between the molecular weights of salts and the lowering of their temperatures of maximum density, and affords for the solutions of a large number of salts the means of calculating the temperature at which they freeze, and also that at which they possess a maximum density. For a large number of bodies also the molecular depression of the temperature of maximum density is nearly four times as great as the molecular depression of the freezing-point. The first of these statements is illustrated as follows:—

	Molecular weight. A.	Coefficient of depression of freezing-point, h.	Molecular depression, A × h.
KCl	74·6	·451	33·6
KBr	119·1	·292	34·8
KI.....	166·0	·212	35·2
KNO ₃ ...	101·0	·267	27·0
NaNO ₃ ...	85·0	·310	26·4

Raoult (*Compt. Rend.* lxxxvii. p. 167) has further shown that the property which anhydrous salts possess of diminishing the vapour-tension of their solutions and of lowering their solidifying-point appears to be inversely as their molecular weights.

Division of a Body between two Solvents.—From his experiments on the division of a body between two solvents, Berthelot (*Ann. Chim. Phys.* [4] xxvi. p. 408) concludes, with regard to the relations between the coefficient of division and the chemical composition of the substance dissolved, that ether removes from water with greater facility:—(1) the more highly carburetted of two homologous acids; (2) a monobasic acid more easily than the corresponding dibasic acid, as acetic rather than oxalic; (3) a monobasic acid rather than a dibasic acid of nearly the same composition, as acetic rather than succinic. (4) Of two acids containing the same proportion of carbon and hydrogen, that with the smallest number of O atoms is the more easily removed, as succinic, $C_4H_6O_4$, rather than malic, $C_4H_6O_5$, or tartaric, $C_4H_6O_6$.

Molecular or Specific Volumes ($= \frac{\text{molecular weight}}{\text{specific gravity}}$).—Schroeder (*Pogg. Ann.* clx. p. 199) has proposed the hypothesis that “All bodies combine in whole volumes.” In the case of gaseous bodies this can be proved, as is well known, without exception by reducing to a common temperature and pressure (Gay-Lussac). For liquids, Kopp (*Ann. Chem. Pharm.* xvi. pp. 153, 303) has shown that the volumes of C, H, O, and other elements in organic liquids are equal in all compounds if their specific gravity be determined at the boiling-point. The following are his more important results:—(1) *Differences of molecular volume are proportional to the differences between the corresponding chemical formulæ.* Thus the difference of CH_2 in homologous series corresponds to a difference of 22 in the molecular volume, thus:—

	Molecular weight.	Specific volume.	Difference.
Formic acid.....	46	42	—
Acetic „	60	64	22
Propionic acid.....	74	86	22
Butyric „	88	108	22

(2) *Isomeric liquids belonging to the same chemical type have equal molecular volumes.* Thus $\begin{matrix} C_2H_3O \\ H \end{matrix} \} O$ and $\begin{matrix} CHO \\ CH_3 \end{matrix} \} O$ have the same molecular volume, viz. 63.4. (3) *Compounds containing as many times two atoms of H less, as others contain*

one atom of C more, have the same molecular volume; or, the volume of one atom of C is equal to that of two atoms of H thus:—

	Molecular weight.	Molecular volume.
$\left\{ \begin{array}{l} \text{C}_8 \text{H}_{18} \\ \text{C}_{10} \text{H}_{14} \end{array} \right.$	114 134	187 187
$\left\{ \begin{array}{l} \text{C}_4 \text{H}_{10} \text{O} \\ \text{C}_6 \text{H}_6 \text{O} \end{array} \right.$	74 94	106·8 106·8

Now it has already been shown that the volume of $\text{CH}_2 = 22$; therefore the specific volume of C = 11, and that of H = 5·5.

In liquids belonging to different types, the volume of the oxygen varies according to the manner in which it is combined in the compound. When the O is joined on to C by one combining power only, its specific volume = 7·8; but when attached to C by both its combining powers, its specific volume = 12·2. A similar thing occurs in the case of S: its specific volume in the former case being 23, and in the latter 28·6. The specific volume of Cl = 22·8, of Br = 27·8, of I = 37·5. N in ammoniacal compounds = 2·3, in cyanides = 17·0, and in nitro-compounds = 17·4. By the use of these constants we may calculate the molecular volume of a compound when its molecular formula is known, thus:—

	Calculated.	Found.
$\text{H}_2 \text{O} = 2 \times 5\cdot5 + 7\cdot8$	= 18·8	18·8
$\text{C}_2 \text{H}_5 \cdot \text{OH} = 2 \times 11 + 6 \times 5\cdot5 + 7\cdot8$	= 62·8	62·5
$\text{CO} \cdot (\text{CH}_3)_2 = 3 \times 11 + 6 \times 5\cdot5 + 12\cdot2$	= 78·2	77·6
$\text{C}_6 \text{H}_5 \cdot \text{NH}_2 = 6 \times 11 + 7 \times 5\cdot5 + 2\cdot3$	= 106·8	106·8

It appears then that the molecular volume depends not only on the chemical composition, but also on the constitution or arrangement of the atoms in the molecules.

As regards the molecular volume of solids, Kopp (*Pogg. Ann.* xlvii. p. 133; lii. pp. 243, 262), and more especially Schroeder (*ibid.* l. p. 552, lii. pp. 269, 282, cvi. p. 226, cvii. p. 113), have endeavoured to show that the hypothesis that all bodies combine in whole volumes holds good not only for gases and liquids, but also in the case of solids; and Schroeder has pointed out that “*equivalent quantities of different elements in uniting with the same quantity of a given element (or compound radical) receive equal increments of volume.*” The explanation of this appears to be that certain elements enter into combination with the same volume which they occupy in the free state. More recently Schroeder (*Deut. chem. Ges.*

* Schorlemmer's ‘Chemistry of the Carbon Compounds.’

Ber. vii. p. 676, ix. p. 1188, x. pp. 848, 1871, xi. pp. 1109, 1142, 2017, 2128, xii. p. 119; *Pogg. Ann.* clx. p. 199) has shown that when an element like silver and a series of its compounds have volumes which stand exactly to one another in simple relations, then they have equal volume-masses or equal steres. Every volume may in fact be represented as a simple multiple of a common volume-mass or stere. So that, *"In every solid compound the volume-measure or stere of one of its elements determines all the other components, and causes equal volume-measures to take up equal steres."* In other words, one of the elements of a compound impresses its own volume-mass or stere on the whole compound, and becomes the controlling element in a whole series of otherwise very different compounds. Thus:—

	Calculated volume.	Observed volume.
$\text{Ag}_1^2 = 2 \times 5.14 = 10.28$	$= 10.28$	$= 10.28$
$\text{Ag}_1^2 \text{Cl}_1^3 = 5 \times 5.14 = 25.70$	$= 25.70$	$= 25.70$
$\text{Ag}_1^2 \text{Br}_1^4 = 6 \times 5.14 = 30.84$	$= 30.84$	$= 30.84$
$\text{Ag}_1^2 \text{I}_1^6 = 8 \times 5.14 = 41.12$	$= 41.12$	$= 41.12$

In the above examples the number of atoms in a compound is indicated in the usual way by a number placed to the right and under side of a symbol, and the number of its steres by a number at the right of the upper side. Here in these compounds it is seen that silver is the predominating element; for it impresses its stere ($= 5.14$) on all the other compounds. By the use of this law Schroeder has endeavoured, with some success, to determine the molecular weight of a solid body; for if substances combine only in whole volumes, then the molecule of a body must contain the number of atoms which are necessary for the components of the compound to fill the space of the whole number of volume-units. He also shows that in many compounds which are capable of existing in more than one form, the difference in form depends on which is the dominating element in the compound. Thus black cinnabar is distinguished from the red by the fact that in the former the mercury stere dominates, whilst in the latter it is the sulphur stere.

[To be continued.]

XLII. *On the Tension of Vapours near Curved Surfaces of their Liquids.* By GEO. FRANCIS FITZGERALD, M.A., F.T.C.D.*

SIR W. THOMSON showed, in the Proc. Roy. Soc. of Edinburgh, Feb. 7, 1870, that the maximum tension of a vapour at the curved surface of its liquid when convex was greater, and when concave less than when flat. He deduced this as a consequence of the ascent of liquids in capillary tubes, by pointing out that the tension of vapour at the top and bottom of the column of liquid differs by the weight of a column of the vapour of that length, while it is impossible to suppose that there can be a continual distillation from the flat surface of the liquid to the curved one in the tube. He does not seem, however, to have observed how this result is connected with the ordinary theories of evaporation; and it is this connexion which I desire to point out.

Assuming, as seems very probable, that evaporation is due to the escape of molecules of the liquid, not from the surface only, but from a very small depth indeed beneath it as well, and that the chances of escape are less the longer the path of the molecule within the liquid, it is at once obvious that a molecule situated at a given depth below the surface will have a much better chance of escape if the surface be convex than if it be flat, and better still than if concave. On the other hand, one tending to enter the liquid from a given depth in the vapour has a less chance of entering a convex surface than a flat one, and still less than of entering a concave one. Hence, in order that the equality between the numbers entering and leaving the surface may be maintained (*i. e.* in order to prevent either evaporation or condensation), the tension of the vapour would have to be greater when in contact with a convex surface than when in contact with a flat, and still greater than when in contact with a concave surface.

If the matter be investigated mathematically, it may be treated very generally indeed if we assume that the depth from which evaporation takes place is so small compared with the radii of curvature of the surface of the liquid, that powers of the ratio of the former to the latter above the first may be neglected. This is certainly legitimate in all cases that can be observed; for the depth from which evaporation takes place must be very small indeed compared with the radii of curvature of the surfaces with which we have to deal.

* Communicated by the Author, having been read at the Meeting of the British Association in Sheffield.

Let, then, the equation of the surface referred to its tangent plane be

$$2z = \frac{x^2}{a} + \frac{y^2}{b} + \dots,$$

where a and b are consequently the principal radii of curvature at the point, and the higher powers of x and y are neglected; for it is obvious that it is only points near the origin that are of any importance. Now z is to be very small, so that x^2 and y^2 are small compared with a and b . Let r be the radius drawn from a point situated on z at a very small distance γ from the origin to any point on the surface, and let θ be the angle between this radius and the normal at the point where it meets the surface, and ψ the angle between this normal and z . A molecule then emitted from γ in the direction r will have to travel a length r within the liquid before escaping. Let then $f(r)$ represent the numbers emitted after having travelled this distance within the liquid. All that we know of $f(r)$ is that it vanishes for all values of r above a very small quantity. Hence we may express the numbers emitted from this point by the double integral

$$n = \iint \frac{f(r) \cos \theta \, dx \, dy}{r^2 \cos \psi};$$

and bearing in mind that a and b are large compared with such values of r , x , y , and γ as do not make $f(r)$ to vanish, we can evidently expand this into the form

$$n = \iint \left(A_0 + \frac{A_1}{a} + \frac{B_1}{b} \right) dx \, dy,$$

where A_0 , A_1 , B_1 are functions of x , y , and γ . It is now to be observed that, on account of the symmetry of the equations involved in x and y , we must have

$$\iint A_1 \, dx \, dy = \iint B_1 \, dx \, dy,$$

and that, consequently, the numbers escaping from this point may be expressed as

$$n = n_0 + n_1 \left(\frac{1}{a} + \frac{1}{b} \right);$$

and if this be integrated for all depths γ , as a and b are the same for each the result must be of the same form, and may be written

$$N = N_0 + N_1 \left(\frac{1}{a} + \frac{1}{b} \right).$$

In this N_0 is the number that would be emitted if the surface were flat; and by changing the signs of a and b we get the

case of a surface curved in the opposite direction. From the considerations mentioned in the beginning of the paper, it is obvious that N_1 must be positive when the surface is convex. In order to obtain the numbers admitted to the surface, we should have to substitute in the foregoing investigation the function corresponding to vapour for $f(r)$ that corresponds to the liquid; but the rest of the investigation is the same, it being recollected that when the surface of the liquid is convex that of the vapour is concave, and *vice versâ*. Hence in the case of a convex liquid surface, we may write those emitted as

$$N = N_0 + N_1 \left(\frac{1}{a} + \frac{1}{b} \right),$$

and those admitted as

$$N' = N'_0 - N'_1 \left(\frac{1}{a} + \frac{1}{b} \right);$$

and for equilibrium we must have

$$N = N',$$

or

$$N'_0 = N_0 + (N_1 + N'_1) \left(\frac{1}{a} + \frac{1}{b} \right).$$

In discussing this result, it is to be observed that an increase in the tension of the vapour probably produces little or no effect upon the numbers emitted; and that consequently N_0 depends only upon the nature and temperature of the liquid; and it is the number that would be emitted or admitted if the surface were flat, and the tension the maximum corresponding to its state. On the other hand, the change in the numbers that would be admitted to a flat surface is proportional to the change in tension at that surface; so that changes in N'_0 are proportional to the changes in tension of the vapour. We thus at once conclude that the maximum tension of vapour in contact with a convex surface of a liquid must be greater than that at a flat one by a quantity which varies directly as the sum of the curvatures of the surface. We know from Sir W. Thomson's investigation, that the coefficient by which this sum of curvatures is multiplied is proportional to the tension of the surface of the liquid; and we can thus connect two apparently unrelated quantities, namely the rate of evaporation with the superficial tension. As $f(r)$ is the only unknown in the foregoing investigation, it might be possible to determine it by observing the rates of evaporation from drops of various sizes. That the tension of the vapour was connected with the sum of the curvatures might also have been suspected from the equilibrium of the surface requiring the normal pressure to vary with this same sum.

XLIII. *The Pseudophone.* By SILVANUS P. THOMPSON, B.A., D.Sc., Professor of Experimental Physics in University College, Bristol*.

THE Pseudophone is an instrument for investigating the laws of Binaural Audition by means of the illusions it produces in the acoustic perception of space. It is therefore the analogue for the ears of the Pseudoscope of Wheatstone, which serves to illustrate the laws of Binocular Vision by means of the illusions it produces in the optical perceptions.

The author has for some months been occupied with an experimental and theoretical investigation of the question of binaural hearing, the chief points hitherto considered being the subjective perceptions of two sounds led separately to the ears, and differing in pitch, phase, or intensity. The results of these investigations were communicated to the British Association in the years 1877 and 1878, and have been published in the *Philosophical Magazine* for 1877 and 1878.

Independently of the work of the author, the theory of Binaural Audition has been attacked by Prof. Anton Steinhauser of Vienna†, who has, however, treated the subject from a somewhat different point of view, and has carefully developed, by geometrical and algebraic reasoning, the laws of the relative intensities with which sound-waves reach the ears from sources of sound situated at various points in front, or at the side, or back, of the observer. In thus calculating according to known geometrical laws the intensities of sounds which reach the ears, Professor Steinhauser neglects such accessory effects as might be produced by differences of pitch, or differences in phase, or diversity of quality of the sounds, and assumes that these have nothing to do with the acoustic perception of the direction in which a sound lies. He assumes that that perception is based solely upon the relative intensities of the two sounds; and upon this assumption his conclusions are indisputable, being simply geometrical deductions from the postulates of the problem.

The author has, however, shown that differences of pitch and of phase play a very important part in the subjective phenomena of audition. His experiments with the simple tones of tuning-forks, which were transmitted by mechanical or electrical means to the ears in such a manner as to produce required differences of phase, showed that difference of phase

* Communicated by the Author, having been read before Section A of the British Association at Sheffield, August 22, 1879.

† *Vide* Steinhauser, "Theory of Binaural Audition," *Phil. Mag.* April and May 1879.

influences the perception of the direction of a sound in a singular manner. Moreover, since the sounds issuing from a point to the right or left of the hearer travel paths of unequal length to the two ears, the difference of phase thereby caused in the two perceived sounds will depend also upon the wavelength of the sound or on its pitch, if simple; and the result will be still more complex if the sound be not a simple tone.

Again, Lord Rayleigh has shown reason for thinking* that the diffraction suffered by the waves of sound as they travel round the head of the observer to the two ears will affect sounds of high and low pitch very unequally, and will therefore still further complicate the perception of the direction of a sound by causing the quality of the sound, if compound, to vary with the position of the head with respect to the direction of the sound-waves, since in different positions the intensities of the high and low components would be differently affected as to their intensity.

These considerations tend to throw some doubt upon the reasonableness of the assumption made by Prof. Steinhauser, in referring the perception of the direction of sounds to the perception of their relative intensities. His conclusions are in fact too general, and can only be considered applicable to certain cases not complicated by questions of pitch or quality, or by the influence of diffraction.

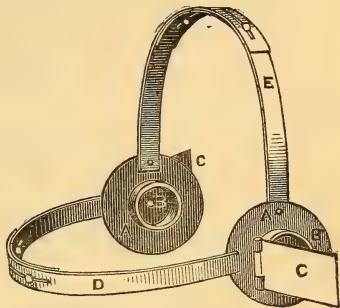
In order to obtain a definite idea of the degree of trustworthiness of the results so carefully elaborated by Prof. Steinhauser, the author undertook a series of experiments on the perception of the direction of sounds of different kinds and pitches, which are not yet concluded—but which at present tend to show the unexpected result that Steinhauser's theory is approximately true for sounds of high pitch only, and not for sounds of medium or low pitch, and that it is more nearly true for sounds in front of or behind the observer than for sounds which reach him obliquely from right or left.

In the course of these researches, it occurred to the author that a simple means of testing some of the main features of the theory was afforded by the possible production of acoustic illusions, making sounds appear to come from other directions than the real source. For if the perception of the direction of a sound depends upon the relative intensities with which it reaches the two ears, and if the intensities with which the sound is perceived in the two ears depend, as Steinhauser assumes, upon the effective magnitude of the pinnae or external flaps of the ears and upon the angles which they make with the line of sight, then any device which should virtually alter either the

* 'Transactions of Musical Association,' 1876.

effective magnitude or the angle of the pinnæ to an amount unknown to the observer must produce a false perception of the relative intensities of the sounds, and give rise to an illusion as to the direction of the sound.

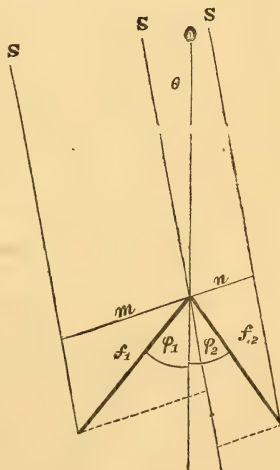
The simple instrument for which the author suggests the name *Pseudophone* consists of a pair of ear-pierces, A A, furnished with adjustable metallic flaps or reflectors of sound, C C, which can be fitted to the ears by proper straps, D and E, and can be set at any desired angle with respect to the axis of the ears, and can also be turned upon a revolving collar about that axis so as to reflect sounds into the ears from any desired direction.



The theory of the *Pseudophone* is very simple, and is as follows:—

The intensity of a perceived sound depends upon the amount of space over which the waves are gathered by the external collecting apparatus of the ear; and by analogy with optical phenomena we may say it depends upon the number of rays of sound which reach the ear.

Let the effective surfaces of the pinnæ which gather the sound-rays be f_1 and f_2 , and let them make equal angles ϕ_1 and ϕ_2 with the line of vision. Let the rays of sound that reach the ears fall in a direction indicated by the lines S, S, S, making an angle θ with the line of sight. The lines m and n , which are drawn perpendicularly, measure the number of sound-rays which reach the pinnæ, and are therefore proportional to the intensities of the sounds which reach the ears.



Now

$$m = f_1 \sin (\theta + \phi),$$

and

$$n = f_2 \sin (\phi - \theta);$$

$$\therefore \frac{m}{n} = \frac{f_1 \sin (\theta + \phi)}{f_2 \sin (\phi - \theta)};$$

and, developing the sines,

$$\frac{m}{n} = \frac{f_1 \sin \theta \cos \phi + \cos \theta \sin \phi}{f_2 \sin \phi \cos \theta - \cos \phi \sin \theta}.$$

Divide by $\cos \theta \cos \phi$, and reckon $f_1 = f_2$.

$$\frac{m}{n} = \frac{\tan \theta + \tan \phi}{\tan \phi - \tan \theta},$$

whence

$$\frac{m+n}{m-n} = \frac{\tan \phi}{\tan \theta},$$

and

$$\tan \theta = \frac{m-n}{m+n} \tan \phi.$$

But

$$\frac{i_1}{i_2} = \frac{m}{n};$$

$$\therefore \tan \theta = \frac{i_1 - i_2}{i_1 + i_2} \tan \phi.$$

Or the difference of the intensities as compared with their sum affords a means of comparing the angle between the line of vision and the direction in which the sounds come, with the angle made by the effective surfaces that receive the rays of sound*.

Such an estimate as we are therefore able to make of the position of a source of sound, judging solely by the relative intensities of the sensation in the two ears, depends upon our previous perceptions and upon our possession of a constant amount of effective auditory surface, and a constant angle subtended between the ears and the line of vision.

In the *Pseudophone* these angles are variable, and the amount of effective surface can also be varied, and this without any knowledge, on the part of the person experimenting with the instrument, as to how much they may be varied. Hence the acoustic illusions which are now to be described.

* This equation, which is the starting-point of Steinhauser's theory, ought more strictly to be interpreted thus: The ratio between the difference of the intensities and their sum is the same as the ratio between the *tangent* of the angle between the line of vision and the direction in which the sounds come and the *tangent* of the angle made by the effective surfaces that receive the sound with the line of vision. Steinhauser assumes, as it is assumed in the paragraph above, that the ratio between the tangents of the angles will be the same in our perception as the ratio between the angles themselves. This is, of course, only true when the angles are very small. Only, unfortunately, for very small angles the perception ceases to be very accurate. No experimental determinations of the degree of accuracy of perception have yet been published.

Suppose one flap to be adjusted at an angle of about 40 degrees with the line of sight, in which position it is about most favourably situated to receive sounds from a point right in front of the observer; then if the other flap be adjusted to any angle greater or less than 40° , fewer rays of sound are reflected into the ear on that side than on the other, and the hearer imagines the source of sound to be situated on that side on which the sensation is more intense. Accordingly, to verify the perception, the hearer turns his head until both ears hear the sound equally loudly, and imagines then that he is looking in the direction of the sound, whereas he is looking at a point situated nearer to that side on which the larger effective surface exists. This observation agrees with Steinhauser's theory. The illusion is very easily obtained by means of a loud-ticking clock, but with some persons does not succeed unless their eyes are blindfolded; for when there is a conflict between the evidence of the eyes and the evidence of the ears, the tendency appears to be to believe the former rather than the latter.

A more striking illusion occurs when the flaps of the pseudophone are reversed and adjusted so as to reflect into the ears sounds which come from immediately behind the observer. In this case also, if a source of sound, situated anywhere behind the head, be observed, if the observer does not know how the flaps are adjusted, he will estimate it to be somewhere in front; and, on turning his head about until the sounds are equally intense, he judges himself to be looking straight at the source of sound, whereas it is in reality exactly in an opposite direction. This illusion succeeds well with a loud-ticking clock, well also with the human voice, but not well with a tuning-fork of medium pitch. In a room the experiment may succeed with a tuning-fork; but there is never the same clear and decisive impression as to the position of the sounding body. In the open air the writer has never succeeded in producing the illusion with a tuning-fork; for the sensation is one of a character from which it appears to be impossible to draw any precise judgment. The sound does not appear to *have* any precise locality. This result, which agrees with some experiments made by Lord Rayleigh with tuning-forks, stands in strong opposition to Steinhauser's theory, which ought, if true at all, to be *à fortiori* true for simple sounds. The author's experiment differs from that of Lord Rayleigh in this respect—that in the case of Lord Rayleigh's experiments with the unaided ears the head of the observer was to be held immovable; whereas in the experiment with the pseudophone the head is turned about, seeking in vain a direction which can be pronounced to be that of the sound-rays.

The illusion succeeds in the open as well as in-doors with the sound of a loud-ticking clock, and with the human voice ; but with shrill sounds it succeeds best, notably with the sharp click of a metronome, and even with a metronome-bell.

These results point to the explanation foreshadowed by Lord Rayleigh, namely that the diffraction of sounds of medium and great wave-length around the head, thus bringing the lower and upper partial tones of the compound sound in unequal intensity to the two ears, plays a great part in our perception of the direction of sounds. When the effects of diffraction are such as to be relatively negligible, as for shrill sounds (whose wave-length is small), then Steinhauser's theory of the *relative intensities* appears to hold good. Any one may at once convince himself of the fact that diffraction *may* thus produce a difference in the relative intensities with which the partial tones of a complex sound reach the ears, by the simple experiment of comparing the *note* of a musically-ticking clock placed in front of the head with its note when placed behind. They appear somewhat different, the difference being one of *timbre* rather than of total loudness.

Another experiment with the pseudophone which gives rise to acoustical illusions, consists in setting one flap to catch sounds from the front, while the other catches sounds from behind or above the observer. Under these circumstances the sounds seem, as the observer moves his head, to come sometimes from the right, sometimes from the left, or sometimes from the ground.

Lastly, most of these experiments with the pseudophone can be repeated simply by holding the hands in front of the ears as flaps ; but here the illusion does not always succeed, as the observer is conscious that his hands are reflecting to the ear sounds from a certain direction, and so the judgment is sophisticated.

XLIV. *A Mode of Exciting an Induction-coil.* By WILLIAM SPOTTISWOODE, M.A., LL.D., President of the Royal Society.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE lately tried a mode of exciting an induction-coil, which I have not seen elsewhere described, and which appears to promise valuable results. It consists in connecting the primary circuit directly with a dynamo- or magneto-machine giving alternate currents. In my own case I have used one of M. de Meritens's excellent machines driven by a three-and-a-half horse-power Otto silent gas-engine. The speed of the

De Meritens machine so driven is about 1300 revolutions per minute.

In this arrangement the "make" currents are of course alternately in one direction and in the other, as also are the "break" currents; so that the discharge appears to the eye during the working of the machine the same at both terminals of the tube.

The advantages of the method are:—first, the fact that, as the machine effects its own make and break, both the contact-breaker and the condenser of the induction-coil can be dispensed with; secondly, that the breaking of the primary, and consequently the delivery of the secondary, currents is perfectly regular; thirdly, that the quantity of the currents in the secondary is very great. With a 20-inch coil by Apps I have obtained a spark about 7 inches in length, of the full thickness of an ordinary cedar pencil. But for a spark of thickness comparable at least with this and of 2 inches length an ordinary 4-inch coil is sufficient.

Owing to the double currents, this spark consists of a bright point at each terminal, and a tongue of the yellow flame, such as is usually seen with thick sparks from a large coil, issuing from each. There is no spark proper during the undisturbed passage of these flames; but if the latter be blown aside, a stream of true bright-line sparks is seen passing between the terminals. This torrent of flame (which, owing to the rapidity with which the currents are delivered by the machine, is apparently continuous) may be maintained for any length of time. It would seem more than probable that this spark may give some very valuable results in spectrum-analysis. The sparks resemble those given by my great coil (described in *Phil. Mag.* 1877, vol. iii. p. 30) with large battery-power and with a mercury break; but with that instrument it is doubtful whether such thick sparks could be produced at short intervals or in a rapid shower as in this case.

In vacuum-tubes, exhausted so as to show bulbous striæ, the effect is excellent. The striæ appear perfectly steady, as with a battery like Mr. Gassiot's or Mr. De la Rue's; and their brilliancy and configuration can be controlled by means of a shunt in the secondary circuit, formed of a column of glycerine and water, so as to diminish at will the amount of current flowing towards the tube.

But I postpone an account of various experiments made with this method until a future occasion.

I am, Gentlemen,

Yours faithfully,

Sevenoaks,
September 20, 1879.

W. SPOTTISWOODE.

XLV. *On a new Standard of Light.* By LOUIS SCHWENDLER*.

[Plate XI. figs. 7-10.]

NO exact measurement of any quantity, even with the most accurate and sensitive test-methods available, can reasonably be expected unless the standard by which the unknown quantity is to be gauged is perfectly *constant* in itself ; or, if nature does not permit of such a desirable state of things, the causes to which the variation of the standard are due should be known, and in addition also their quantitative effect on the standard, in order to be able to introduce a correction whenever accuracy of measurement should permit and circumstances necessitate it.

This requirement for a standard necessarily entails on the one hand a knowledge of the relations which exist between the standard and the causes of its variation, and on the other hand the possibility of an accurate and independent measurement of these causes.

Further, having no constant standard, it is impossible to produce two quantities of the same kind bearing a fixed and known ratio to each other ; consequently no idea can be formed of the accuracy of the test-method adopted ; and if such is impossible, we are also unable to improve the test-method in itself, *i. e.* with respect both to accuracy and sensitiveness.

The inconstancy of a standard acts therefore perniciously in two directions : it prevents us from being able to execute accurate measurements even with the most accurate and sensitive test-methods, supposing such are available, and, further, leaves us in the deplorable condition of not being able to improve the test-method although we may be convinced that the method of testing requires such improvement.

It may be safely asserted that in any of the branches of the physical sciences where constant standards do not exist the progress in accurate knowledge of nature must be slow, if not impossible.

This train of thought will, I think, invariably beset the physicist who endeavours to make photometric measurements.

Recent experiments on the value of the electric light as compared with the ordinary means of illumination † called my attention forcibly to this point.

* From the Journal of the Asiatic Society of Bengal, vol. xlviii. part ii. 1879. Communicated by the Author.

† These experiments I had to institute on behalf of the Board of Directors of the East-Indian Railway Company, under orders of the Secretary of State for India to inquire into the feasibility and practicability of lighting up Indian Railway-stations by the electric light.

Old Standards for Light-measurements.—Up to the present in England the *Standard Candle* * has been adopted as the standard of light, the unit of light being defined as that light which the said candle emits when burning steadily at a certain definite rate. In France the *Carcel Burner* (bec Carcel) has been introduced as the standard of light, the unit of light in this case being defined as that light which emanates from a good moderator lamp burning pure colza oil at a given definite rate. The ratio of these two arbitrary units is given by several authorities very differently, the mean value being about

10 standard candles = 1 Carcel burner.

These two standards of light, although answering perhaps certain practical requirements, are by their nature ill-adapted to form the units of light-intensities. A good and trustworthy standard should possess absolute constancy, or, if not, should afford the possibility of application of a correction for the variation, and moreover should be capable of accurate reproduction. These qualifications are certainly not possessed by the standards at present in use.

A candle of whatever compound and size will partake of something of the nature of a complex body, an accurate reproduction of which must always be a matter of great difficulty. Exactly the same holds good for the Carcel burner.

Further, the amount of light these standards produce depends to a very considerable extent on external influences, which do not allow of easy control or measurement, and which therefore cause variations in the standard light for which it becomes impossible to introduce a correction. For instance, the rate and regularity with which a candle burns and the amount of light it gives depend, in addition to the material of which the candle consists, on the ready and regular access of oxygen. In a closed-up place, like the box of a photometer, if the draught is not well regulated or the supply of fresh air not quite constant, it can be easily observed that the very same candle may emit light at different times varying as much as 50 per cent. Another difficulty is introduced by the variation of the length of the wick, and of the candle itself, by which the standard light necessarily alters its position in the

* The Metropolitan Gas Act, 1860 (23 and 24 Vict. cap. 125, sect. xxv.) defines the standard candle as:—"Sperm candles of 6 to the pound, each burning 120 grains an hour." I have tried the standard candles as made by two different manufacturers, Messrs. Field and Co. and Mr. Sugg. These candles are sold as six to the pound, and consume, according to my own experiments, about 8.26 grams per hour when placed in a large room and direct draughts excluded.

photometer, and consequently its quantitative effect on a given point. These difficulties might be overcome to a certain extent by mechanical means—as, for instance, by cutting the wick automatically within equal and short intervals of time, and by placing the candle in a closely fitting metal tube, against the top rim of which a spring presses the burning candle—in fact, a similar construction to that used for carriage-candles. But, to say the least, all such arrangements are cumbersome. Without going into further details with reference to the Carcel burner, it may be said that the disadvantages of this standard are at least equally great. In fact it appeared to me that the production of a standard light by combustion is not the right method; the flame resembles too much organic life with its complex and incessantly varying nature. Gauging mechanical force by the power a particular horse of a certain breed is able to exert, can scarcely be called a less scientific standard than the combustion standard for measuring light. Under these circumstances I thought it best to leave the old track, and produce the standard of light *by the heating effect a constant current has in passing through a conductor of given mass and dimensions**.

New Standard of Light.—Several platinum photometric standards were made and tried. If the current passing through the platinum was kept constant, the light produced was also constant; and for the same current and the same platinum standard the light was always of the same intensity, under whatever other circumstances the experiments were conducted.

Platinum evidently is the best metal which can be chosen; for it does not change in contact with oxygen, it can be procured very pure, and its melting-point is high enough to allow an intense light.

It is probable that at a high temperature platinum becomes volatilized; but this process can only be exceedingly slow, and

* The idea of using the light produced by a conductor through which a strong current passes as the unit of light appeared to me so natural and simple, that I could scarcely understand why it had not been proposed and acted upon before.

I could, however, find nothing on the subject anywhere, until lately my attention was called to a small pamphlet written by Zöllner in 1859, in which the same idea occurs. In the preface to his Inaugural Dissertation, Zöllner says:—"Andererseits aber auch zu zeigen, dass ein galvanisch glühender Platindraht von den bis jetzt bekannten Lichtquellen zur Aufstellung einer photometrischen Einheit, trotz mancher practischer Schwierigkeiten, vielleicht dennoch das geeignetste Mittel sei."

I have since learnt that Dr. Draper, as early as 1844, proposed a "unit lamp" consisting of a platinum strip heated by an electric current.

therefore the light produced by a standard cannot alter perceptibly in time. To make the light constant from the moment the current passes, *i. e.* to establish dynamic equilibrium between the heat produced and the heat lost per unit of time, it is necessary to make the arrangement in such a manner that the electric resistance offered by the standard is only in the piece of platinum intended to be made hot by the current, and not in the other parts of the circuit.

For this reason I find it best to cut the piece of platinum out of a platinum sheet.

Figure 7, Plate XI. gives the form in actual size. The two ears, left white in the drawing, may then conveniently form the electrodes between the leading wires and the piece of U-shaped platinum which has to produce the light. As the U-shaped portion is left in its natural connexion with the ears, the contact takes place over a large surface; and therefore the contact resistance must be small. This special form, if the dimensions are defined as well as the weight of the platinum sheet out of which it is cut, can be easily reproduced anywhere. Further, it is required to exclude the draught from the heated platinum. This is best done by putting on a cover of thin white glass. One half of it is left white; the other half is blackened on the inside. This precaution is required in order to ensure that light emanating from one side only of the platinum is used in the photometer; otherwise light from the back part of the heated platinum would be reflected into the photometer. This part is unknown, and therefore could not be taken into account when measuring the light emanating from one side of another light. In fact, to be able to form right conclusions from photometric measurements, it is necessary to arrange the experiment in such a manner that either the two lights under comparison throw the same fraction of the total light into the photometer, or, if this is impossible, to ascertain this proportion accurately.

The platinum light-standard (P. L. S.), described before, we will call in future A. Sending a current of 6.15 webers through it (15° deflection on my large tangent galvanometer, for which the constant = 2.296 C. G. S.), the P. L. S. (A) produces a light equal to 0.69 Sugg's candle, or,

$$1 \text{ Sugg's candle} = 1.44 \text{ P. L. S. (A) with } 6.15 \text{ webers.}$$

Hence, if this particular light were adopted as the unit, we might define it as follows:—

6.15 webers passing through a piece of platinum 2 millims. broad, 36.28 millims. long and 0.017 millim. thick, weighing

0.0264 grm., having a calculated resistance = 0.109 S. U. and a measured resistance = 0.143 S. U. at 66° F., gives the unit for light-intensity*.

Photometric Measurements.—Having now a constant light, it became possible to measure the variations of light which the combustion standards invariably show.

For instance, one of Sugg's candles was compared with the P. L. S. (A) with the result shown in the following table:—

Distance in millimetres.		Remarks.
P. L. S. (A) with 6.15 webers.	Sugg's candle.	
millim.	millim.	
100	117	The P. L. S. (A) was kept at the same position = 100 millims.
	120	
These readings were taken in about five minutes.	112	Sugg's candle was moved in order to get the light equal.
	110	
	120	
	120	
	120	The variations observed were actually in the candle and not in the platinum standard, as the eye could easily discern.
	120	
	126	
	128	
	117	
	120	
	123	
	127	

This gives as an average:—

1 Sugg's candle = 1.44 P. L. S. (A) with 6.15 webers.

* In order to show that a platinum light-standard can easily be reproduced, I will give here some actual measurements:—

The platinum sheet out of which the P. L. S. (A) was cut weighed 0.0364 gram per square centimetre. From this the weight of the part which becomes hot, calculated, gives 0.0264 gram. The resistance of the standard, measured at 66° F., gave 0.143 S. U., including contact resistances.

Now another piece of platinum sheet 26 × 28 millims. was found to weigh 0.265 gram. The piece cut off which actually becomes hot = 0.026 gram, which agrees within 0.0004 gram with the weight found by calculation for the P. L. S. (A) actually used.

Taking the specific resistance of mercury = 96190 }
 " " of platinum (annealed) = 9158 } at 0° C.,
 S. U.

the calculated resistance of the platinum which becomes hot = 0.109 } at
 measured resistance, including contact resistance. = 0.143 } 66° F.;
 or contact resistance probably = 0.034 S. U.

It is therefore much more accurate to define the P. L. S. by weight than by resistance.

$\frac{\text{Max.}}{\text{Min.}} = \frac{1.64}{1.21}$, or total variation of the candle about 30 per cent. from the average in the very short interval of time of about five minutes. This needs no further comment. Some additional experiments were made in order to ascertain the variation of the light of a standard candle.

The P. L. S. (B)* with a current=5.9 webers was used as unit.

1st candle, 7 readings in 10 minutes.

Mean=1.08 P. L. S. (B) ;

$\frac{\text{max.}}{\text{min.}} = \frac{1.19}{1.00}$, or total variation=17.6 per cent.

The maximum was obtained directly after having opened the photometer, when fresh air entered.

2nd candle, 10 readings in 14 minutes.

Mean=1.07 P. L. S. (B) ;

$\frac{\text{max.}}{\text{min.}} = \frac{1.32}{0.69}$, or total variation=59 per cent.

The minimum was obtained directly after freshly lighting the candle.

3rd candle, 12 readings in 24 minutes.

Mean=1.07 P. L. S. (B) ;

$\frac{\text{max.}}{\text{min.}} = \frac{1.30}{0.81}$, or total variation=46 per cent.

The lowest reading was obtained shortly after lighting the candle.

4th candle, 14 readings in 22 minutes.

Mean=0.94 P. L. S. (B) ;

$\frac{\text{max.}}{\text{min.}} = \frac{1.26}{0.58}$, or total variation=72 per cent.

The lowest reading cannot be accounted for.

Two new platinum light-standards, of the same form and size as the P. L. S. (A) described before, were placed in circuit of eight Grove's cells connected up successively and with a mercury rheostat in circuit, to keep the needle of the tangent galvanometer at a constant deflexion.

These two new P. L. S., called II. and III., were placed in the photometer to compare their lights and by it test the

* This platinum standard (B) was the first made and has a different form from the other (A) described. Dimensions and weight cannot be accurately given now.

accuracy of the photometer-readings, and other influences to be named further on (see fig. 8).

$$d + d' = D = 250 \text{ millims. (constant).}$$

Light i produced by P. L. S. (III.); light i' produced by P. L. S. (II.),—the balance between the two lights being obtained by moving the prisms within that fixed distance. A piece of red glass was used for taking the readings.

In the following Table the results are given :—

No. of experiment.	P. L. S.		Deflection on tangent-galvanometer.	$\frac{i'}{i}$	Remarks and Particulars.
	II. producing i' d' millims. from prism.	III. producing i d millims. from prism.			
1.	100	150	18.8		Both lights having glass covers; but glasses were quite clear.
	100	150	18.8		
	100	150	18.8		
	100	150	18.8		
	100	150	18.8		
	99	151			
	100	150			
	99.86	150.14	18.8	0.44	
2.	103	147	18.8		A clear glass cover on No. III.; no glass cover on No. II.
	102	148	18.8		
	102	148			
	103	147			
	102	148			
	102.4	147.6	18.8	0.48	
3.	98	152	18.8		A clear glass cover on No. II.; no glass cover on No. III.
	97	153	18.8		
	98	152	18.8		
	98	152			
	98	152			
	99	151			
	98	152			
	98	152	18.8	0.42	
4.	98	152	18.8		A glass cover on No. III., the back of it covered inside with black paper; a clear glass cover on No. II.
	98	152	18.8		
	99	151	18.8		
	100	150	18.8		
	100	150			
	98	152			
	98.83	151.17	18.8	0.43	

Table (continued).

No. of experiment.	P. L. S.		Deflection on tangent-galvanometer.	$\frac{i'}{i}$.	Remarks and Particulars.
	II. producing i' d' millims. from prism.	III. producing i d millims. from prism.			
5.	101	149	18·8		Both lights covered up with glass covers, each glass cover having inside a black paper.
	101	149	18·8		
	102	148	18·8		
	101	149			
	101	149			
	101·2	148·8	18·8	0·46	
6.	103	147	21		Current increased by decreasing the resistance of the mercury rheostat, but kept constant at 21°. Clear glass again on both, like experiment No. I.
	101	149	21		
	101	149	21		
	101	149	21		
	101	149	21		
	101	149	21		
	101·3	148·7	21	0·46	
7.	104	146	21		Clear glass cover on No. III. No glass cover on No. II.
	103	147	21		
	102	148	21		
	102	148	21		
	102	148	21		
	102	148	21		
	102·6	147·4	21	0·48	
8.	101	149	21		Clear glass cover on No. II. No glass cover on No. III.
	100	150	21		
	100	150	21		
	99	151	21		
	100	150			
	100	150			
	100	150	21	0·44	
9.	101	149	21		Both the clear glass covers on.
	101	149	21		
	101	149	21		
	101	149	21	0·46	

The deflection 18°·8 represents a current = 7·82 webers.

The deflection 21° represents a current = 8·81 webers.

From these results the following conclusions can be drawn:—

The thin glass covers, as was to be expected, absorb a

measurable quantity of light. Compare the results of experiments 1, 2, and 3, and of 6, 7, 8, and 9.

Covering the glass covers inside with black paper to avoid back-reflection, appears to weaken the light, as was to be expected. Compare the results of experiments 1, 4, and 5.

The ratio $\frac{i'}{i}$ of the two lights is independent of the strength of the current, which it ought to be.

These results, although showing nothing extraordinary, *i. e.* what could not have been foretold without making the experiments, are nevertheless valuable, since they prove, in the first place, that thin glass covers take away very little light, and that back-reflection is also very little; but small as these influences are, they have been unerringly measured by the photometer, showing this instrument to be very accurate and the eye quite trustworthy. That the light i , produced by P. L. S. III., was so much more intense than i' , produced by P. L. S. II., is due to the fact that the platinum sheet out of which no. II. was cut was much thicker than the other.

Detailed Description of the Standard and the Method of using it.

Fig. 9, Plate XI., gives the construction of the platinum standard in half its natural size. I need not give further explanation on this point, as every thing will be readily understood from the drawing.

Fig. 10 shows the diagram of the connexions:—

P. L. S. is the standard.

G, a current-indicator, or, better, current-measurer. The deflecting-ring must consist of a few convolutions of thick copper wire, of no perceptible resistance. The small magnet needle is best pivoted, carrying a long aluminium index.

E is the battery, consisting of a few elements of high E. M. F. and low internal resistance connected up successively. Grove's, Bunsen's or large Daniell's cells will answer well for the purpose.

(1) is a stopper, by which the circuit can be conveniently opened or closed.

M is a mercury rheostat of about one unit resistance. A groove of about 1 millim. section and 1 metre total length is cut in hard wood (not ebonite, as mercury does not run well in ebonite). The hard wooden board is supported by three levelling-screws.

Further, the mercury is in perfect metallic contact with two iron terminals, *ff*. These terminals are not to be fixed to the board. They are simply placed in the mercury, which fills small reservoirs at each end of the mercury thread.

The resistance of the mercury rheostat can be easily altered by moving the bridge b along the two parallel mercury-grooves. If the bridge is taken out, the total resistance of the rheostat is in circuit.

If the bridge b is close to the two terminals ff , the resistance of the rheostat is *nil*.

This range of resistance with about 6 to 10 volts will prove sufficient to make the current strong enough and to keep it constant for many hours, especially if the precaution be taken to open the circuit when no light is required. The bridge b consists of a strip of copper at least 2 centims. broad and 1 millim. thick. The knife-edges which dip into the mercury are amalgamated.

The current-measurer G has been gauged by comparison with a standard tangent-galvanometer; so that the currents indicated by certain deflections of the needle are correctly known in absolute measure.

Whenever a photometric measurement is made, the current is adjusted to its defined strength; *i. e.* the given known deflection is procured by moving the copper bridge b .

If the instrument G is well constructed, this adjustment of current-strength can be executed as accurately as weight-measurement by a chemical balance.

Correction for the Standard.

Although with the above arrangement it will be always possible to keep the current constant and up to its defined amount, it might nevertheless happen under particular circumstances that the current producing the light has been rendered different from the current for which the standard has been defined.

In this case the following correction can be applied:—

$$I = \frac{1}{(1 + \alpha) \left(\frac{c}{\gamma}\right)^2 - \alpha},$$

where c is the current for which the intensity of the light has been defined as unity, γ the actually observed current, and α the coefficient for platinum which gives the percentage variation of resistance at high temperature, 1500° – 2000° F. for 1° Celsius.

This correction has been developed on the supposition that the light produced in the given piece of platinum is proportional to the work done by the current through the resistance of the platinum, and further, that, temperature and light

are proportional. These suppositions are almost correct for small variations of the current.

In conclusion it may be stated that it was ascertained that the platinum light-standard (B) produced the unit intensity of light (the unit of light equal to the light emitted by the standard candle) at a total expenditure of energy equal to 427 Ω ergs per second. Of these, 300 Ω ergs were actually transformed into light by heating the platinum up to a high temperature; while the remaining 127 Ω ergs were lost for illuminating-purposes, being used for raising the temperature of the circuit exclusive of the platinum standard.

The platinum light-standard (A) being made of much thicker platinum sheet, showed a much less favourable result. The unit of light by (A) was produced at a total expenditure of energy equal to 1226 Ω ergs per second, of which 725 Ω ergs were actually transformed into light; the remaining 501 Ω ergs were wasted in heating the circuit to low temperature (no light). Considering that the unit of light can be produced in an electric arc at a total expenditure of energy of 10 Ω ergs per second only (see my 'Précis of Report on Electric-Light Experiments,' London, 1st Nov. 1878, p. 11), when produced by Siemens's intermediate dynamo-electric machine, it follows that, from an engineering point of view, light by *incandescence* can scarcely be expected to compete with light by *disintegration* (electric arc).

In fact, it appears that light by *incandescence* is scarcely any cheaper than light by *combustion*. The reason for this is that the temperature of an incandescent platinum wire is not very much higher than the temperature of a flame, and that for unit volume the mass which has to be kept heated in a piece of platinum is much larger than the mass in a flame. Unless we should be fortunate enough to discover a conductor of electricity with a much higher melting-point than platinum, and that the specific weight and specific heat of that conductor is also much lower than for platinum, and that at the same time the new conductor does not combine at high temperatures with oxygen, we can scarcely expect that the principle of incandescence will be made use of for practical illumination.

Further, it was ascertained that the resistances of the platinum light-standards (not including contact resistance) were as follows:—

P. L. S. (B) = 0.136 ohm at 22°·2 C.

= 0.876 „ at the temperature of the standard

where the light was measured, or increase $\frac{0.876}{0.136} = 6.44$.

P. L. S. (A) = 0.102 ohm at 18°·9 C.

= 0.964 ,, at the temperature of the standard

where the light was produced, or increase $\frac{0.964}{0.102} = 9.45$.

I regret that I have not been able to calculate from the above results the temperature of the heated platinum, since I could not procure in time a copy of Dr. William Siemens's Bakerian Lecture (1871), which at present, to my knowledge, is the only source whence the increase of resistance of platinum at high temperatures can be found.

To sum up, the advantages of the new standard of light are:—The light is perfectly constant if the current be kept constant; it allows a correction to be made for the variation of the current if this variation is known; it can be reproduced accurately everywhere if ordinary precautions be taken to secure pure platinum*; its magnitude can be altered to any extent to suit certain practical purposes by simply varying the elements of weight, shape, and size of the platinum, or the strength of the current passing through it; it does not alter of itself either in intensity, size, or position, and therefore by it most accurate photometric measurements can be executed; the standard can be easily made to fit into any adopted system of absolute units. Hence the new standard fulfils *all* the recognized conditions of a perfect and rational standard; and therefore it would be advisable to adopt it in future as the practical standard for light-measurement. There would be no practical difficulties met with in the introduction of the new standard for technical purposes.

XLVI. *Investigations in Optics, with special reference to the Spectroscope.* By LORD RAYLEIGH, F.R.S.

[Continued from p. 274.]

§ 4. *Influence of Aberration.*

IN the investigations of § 2 the wave-surface was considered to be plane, or (after passing through a condensing lens) spherical. As all optical instruments are liable to aberration, it is important to inquire what effects are produced thereby upon the

* The conductivity of any metal is much lowered by slight impurities; and platinum does not form an exception; hence great care must be exercised in the selection of platinum for the light-standard. Dr. William Siemens, in his Bakerian Lecture says:—"The abnormal resistance of some platinum is due chiefly to the admixture of iridium or other metals of the same group; and it appears that platinum prepared by the old welding process is purer, and therefore better suited for electrical purposes, than the metal consolidated by fusion in a Deville furnace."

intensity-curves, and especially to ascertain at what point a sensible deterioration of definition ensues. The only work bearing upon the present subject with which I am acquainted is Sir G. Airy's investigation "of the intensity of light in the neighbourhood of a caustic"* ; but the problem considered by him relates to an *unlimited* beam.

Considering in the first place the case of a beam of rectangular section, let us suppose that the aberration, or error of phase, is the same in all vertical lines, so that the actual wave-surface is cylindrical. With origin at the centre and axis of x horizontal, the aberration may be expressed in the form

$$cx^3 + fx^4 + \dots \quad (1)$$

No terms appear in x or x^2 : the first would be equivalent to a general turning of the beam ; and the second would imply imperfect focusing of the central parts. In many cases the circumstances are symmetrical with respect to the centre ; and then the first term which occurs is that containing x^4 . But in general, since the whole error of linear retardation which we shall contemplate is exceedingly small in comparison with other linear magnitudes concerned in the problem, the term in x^3 is by far the more important, and those that follow may be neglected.

As in the case of no aberration (treated in § 2), the distribution of brightness in the image of a point is similar along every vertical line in the focal plane ; and therefore the image of a vertical line follows the same law of brightness as applies in the case of a point to positions situated along the axis of ξ . The phase of the resultant at any point ξ is by symmetry the same as that of the secondary wave issuing from the centre ($x=0$) ; and thus the amplitude of the resultant is proportional to

$$\int_0^{+\frac{1}{2}a} \cos 2\pi \left(\frac{x\xi}{\lambda f} + cx^3 \right) dx. \quad (2)$$

In Sir G. Airy's problem the upper limit of the integral (2) is infinite. Fortunately for my purpose the method of calculation employed by him is that of quadratures, and the intermediate results are recorded (p. 402) in sufficient detail. In order to bring (2) into conformity with Airy's notation, we must take

$$2\pi cx^3 = \frac{1}{2}\pi\omega^3, \quad \frac{2\pi x\xi}{\lambda f} = -m\frac{\pi}{2}\omega ; \quad (3)$$

we thus obtain

$$(4c)^{-\frac{1}{3}} \int_0^{\frac{1}{2}a \cdot \sqrt[3]{4c}} \cos \frac{1}{2}\pi(\omega^3 - m\omega) d\omega, \quad (4)$$

* Cambridge Phil. Trans. vol. vi. 1838.

in which the upper limit of the integral is the cube root of the extreme aberration expressed in quarter-periods. For example, the upper limit is unity when the phase at one extremity is a quarter-period in advance, and that at the other extremity a quarter-period in the rear, of the phase at the centre.

The influence of aberration may be considered in two ways. We may suppose the aperture (a) constant, and inquire into the effect of an increasing aberration (c); or we may take a given value of c (*i. e.* a given wave-surface), and examine the effect of a varying aperture. To the latter comparison Airy's results are more immediately applicable. The following Table, easily derived from that given by him, exhibits the values of $\int \cos \frac{1}{2} \pi (\omega^3 - m\omega) d\omega$, between the limits specified in the headings of columns 2, 3, 4, 5. The results are applicable at once to the comparison of the amplitude-curves corresponding to various apertures, since the relation of m to ξ in (3) is independent of a . To obtain intensities, it would be necessary to square the numbers given in the Table.

Value of m .	From 0 to 1.00.	From 0 to 1.26.	From 0 to 1.44.	From 0 to ∞ .
-4.0	+0929	-0692	+0588	+0030
-3.6	0783	-0467	+0197	0062
-3.2	+0343	+0142	-0309	0124
-2.8	-0203	0849	-0461	0239
-2.4	0563	1320	+0018	0444
-2.0	-0430	1399	1009	0791
-1.6	+0411	1263	2095	1346
-1.2	1997	1377	2906	2184
-0.8	4140	2266	3462	3362
-0.4	6449	4185	4211	4886
0.0	8422	6873	5672	6653
+0.4	9570	9538	7898	8404
+0.8	9559	11120	10157	9701
+1.2	8307	10748	11141	9979
+1.6	6024	8170	9681	8705
+2.0	3161	+3952	+5569	5649
+2.4	+0303	-0679	-0060	+1172
+2.8	-1988	4290	5110	-3624
+3.2	3309	5826	7545	7087
+3.6	3521	5028	6485	7652
+4.0	-2761	-2525	-2725	-4745

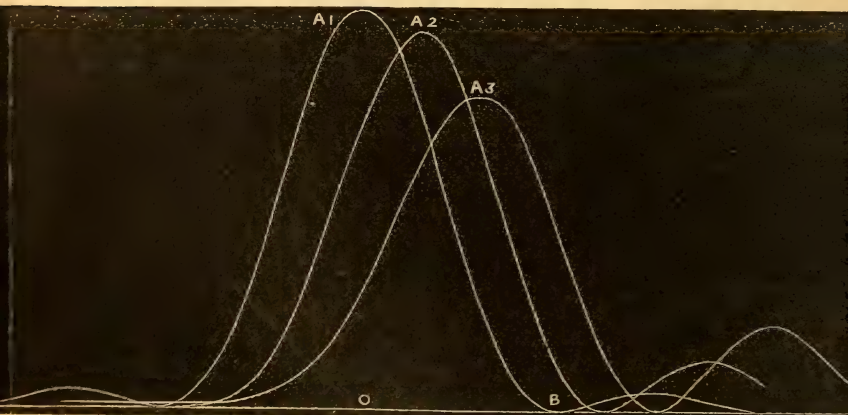
The second column relates to the case where the aperture is such that the aberration between the extremities and the centre is one quarter of a period, or (which is the same thing) where the wave-surface at the extremities deviates by a quarter wavelength from the tangent plane at the central line of inflection. It will be seen that the position of maximum illumination deviates sensibly from the centre ($m=0$, $\xi=0$). This is no more than might have been expected, since the plane which most nearly coincides with the actual wave-surface is inclined to the cen-

tral tangent plane. The third column relates to an aperture about a fourth part larger, for which the extreme aberration is $(1.26)^3$ quarter-periods or nearly *half* a period, and the fourth column relates to an aperture such that extreme aberration amounts to about three quarters of a period.

From columns 2 and 3 we see that an increase of aperture up to that corresponding to an extreme aberration of half a period has no ill effect upon the central band, but it increases unduly the intensity of the first lateral band at $m = +3.2$. Indeed the principal objection to much greater apertures is this augmented importance of the lateral band. The practical conclusion is that the best results will be obtained with an aperture giving an extreme aberration of from a quarter to half a period, and that with an increased aperture aberration is not so much a direct cause of deterioration as an obstacle to the attainment of that improved definition which should accompany the increase of aperture.

We will now suppose the aperture given, and examine the effect of increasing aberration. In applying the tabular results we must have regard to the factor $(4c)^{-\frac{1}{3}}$, which occurs in (4), and we must take account of the variation of the relative scale of m and ξ in passing from one curve to another ($\xi \propto m^3 \sqrt{4c}$). The results for three cases are expressed graphically by the curves in fig. 6. The first, which rises highest, represents

Fig. 6.



the *intensity* at the various points of the focal plane when there is no aberration—the same as in fig. 1. The second and third curves represent the intensities when the extreme aberrations are a quarter period and half a period respectively. The total areas of these curves are the same, since the whole

quantity of light in the beam is independent of the aberration; and this area is equal to that of a rectangle whose height is the maximum ordinate OA of the first curve, and width the distance OB between O and the first position of zero intensity. It appears that aberration begins to be distinctly mischievous when it amounts to about a quarter-period, *i. e.* when the wave-surface deviates at each end by a quarter wave-length from the true plane. The most marked effect is the increased importance of the lateral band on one side, and the approximate obliteration of the lateral band on the other side.

When the aberration is symmetrical about the centre of the beam, the term in x^3 vanishes, and the whole effect is of higher order. In general the term in x^4 will preponderate; and thus the problem for a symmetrical beam resolves itself into the investigation of aberration varying as x^4 . In one respect the problem is simpler than the preceding, on account of the symmetry of the intensity-curves; but in another it is more complicated, since the phase of the resultant does not correspond with that of the central element. The intensity is represented by

$$\left[\int_{-\frac{1}{2}a}^{+\frac{1}{2}a} \cos 2\pi \left(\frac{x\xi}{\lambda f} + fx^4 \right) dx \right]^2 + \left[\int_{-\frac{1}{2}a}^{+\frac{1}{2}a} \sin 2\pi \left(\frac{x\xi}{\lambda f} + fx^4 \right) dx \right]^2, \quad . \quad . \quad . \quad (5)$$

and requires for its calculation two integrations. These could be effected by quadratures; but the results would perhaps scarcely repay the labour, especially as the practical question differs somewhat from that here proposed. The intensity-curve derived from (5) represents the actual state of things on the supposition that the focusing adopted is that proper to a very small aperture; whereas in practice the aberration would be in some degree compensated for by a change of focus, as it is obvious that the real wave-surface, being curved only in one direction, could be more accurately identified with a sphere than with a plane.

Some idea of the effect of aberration may be obtained from a calculation of the intensity at the central point ($\xi=0$), where it reaches a maximum; and this can be effected without quadratures by the aid of a series. In this case we have instead of (5),

$$4 \left[\int_0^{\frac{1}{2}a} \cos (2\pi f x^4) dx \right]^2 + 4 \left[\int_0^{\frac{1}{2}a} \sin (2\pi f x^4) dx \right]^2. \quad . \quad (6)$$

Now by integration by parts it can be proved that

$$\int_0^x e^{ihx^4} dx = e^{ihx^4} \left\{ x - 4ih \frac{x^5}{5} + \frac{(4ih)^2}{5} \frac{x^9}{9} - \frac{(4ih)^3}{5 \cdot 9} \frac{x^{13}}{13} + \dots \right\},$$

whence by separation of real and imaginary parts, and putting x equal to unity,

$$\int_0^1 \cos(hx^4)dx = \cos h \left\{ 1 - \frac{(4h)^2}{5 \cdot 9} + \frac{(4h)^4}{5 \cdot 9 \cdot 13 \cdot 17} - \dots \right\} \\ + \sin h \left\{ \frac{4h}{5} - \frac{(4h)^3}{5 \cdot 9 \cdot 13} + \frac{(4h)^5}{5 \cdot 9 \cdot 13 \cdot 17 \cdot 21} - \dots \right\}, \quad (7)$$

$$\int_0^1 \sin(hx^4)dx = \sin h \left\{ 1 - \frac{(4h)^2}{5 \cdot 9} + \frac{(4h)^4}{5 \cdot 9 \cdot 13 \cdot 17} - \dots \right\} \\ - \cos h \left\{ \frac{4h}{5} - \frac{(4h)^3}{5 \cdot 9 \cdot 13} + \frac{(4h)^5}{5 \cdot 9 \cdot 13 \cdot 17 \cdot 21} - \dots \right\}. \quad (8)$$

Calculating from these series I find

$$\int_0^1 \cos(\tfrac{1}{4}\pi x^4)dx = \frac{1 \cdot 36704}{\sqrt{2}}, \quad \int_0^1 \sin(\tfrac{1}{4}\pi x^4)dx = \frac{21352}{\sqrt{2}}, \\ \left[\int_0^1 \cos(\tfrac{1}{4}\pi x^4)dx \right]^2 + \left[\int_0^1 \sin(\tfrac{1}{4}\pi x^4)dx \right]^2 = \cdot 9576.$$

Again,

$$\int_0^1 \cos(\tfrac{1}{2}\pi x^4)dx = \cdot 87704, \quad \int_0^1 \sin(\tfrac{1}{2}\pi x^4)dx = \cdot 26812, \\ \left[\int_0^1 \cos(\tfrac{1}{2}\pi x^4)dx \right]^2 + \left[\int_0^1 \sin(\tfrac{1}{2}\pi x^4)dx \right]^2 = \cdot 84109.$$

Again,

$$\int_0^1 \cos(\pi x^4)dx = \cdot 64357, \quad \int_0^1 \sin(\pi x^4)dx = \cdot 33363, \\ \left[\int_0^1 \cos(\pi x^4)dx \right]^2 + \left[\int_0^1 \sin(\pi x^4)dx \right]^2 = \cdot 52549.$$

Thus an extreme aberration of one eighth of a period reduces the intensity at the central point from unity, corresponding to no aberration, to $\cdot 9576$. With an aberration of one quarter of a period the intensity is $\cdot 84109$; and with an aberration of half a period the intensity is reduced to $\cdot 52549$. We must remember, however, that these numbers will be sensibly raised if a readjustment of focus be admitted.

In most optical instruments other than spectroscopes the section of the beam is circular, and there is symmetry about an axis. The calculation of the intensity-curves as affected by aberration could be performed by quadratures from tables of Bessel's functions; but, as in the case last considered, the results are liable to a modification in practice from readjustment of focus. For the central point we may obtain what we require from a series.

The intensity may be represented by

$$[2 \int_0^1 \cos(hr^4) r dr]^2 + [2 \int_0^1 \sin(hr^4) r dr]^2,$$

the scale being such that the intensity is unity in the case of no aberration ($h=0$). As before, we find

$$2 \int_0^1 e^{ihr^4} r dr = e^{ih} \left\{ 1 - \frac{4ih}{6} + \frac{(4ih)^2}{6 \cdot 10} - \frac{(4ih)^3}{6 \cdot 10 \cdot 14} + \dots \right\},$$

whence

$$2 \int_0^1 \cos(hr^4) r dr = \cos h \left\{ 1 - \frac{(4h)^2}{6 \cdot 10} + \frac{(4h)^4}{6 \cdot 10 \cdot 14 \cdot 18} - \dots \right\} \\ + \sin h \left\{ \frac{4h}{6} - \frac{(4h)^3}{6 \cdot 10 \cdot 14} + \dots \right\}, \quad (9)$$

$$2 \int_0^1 \sin(hr^4) r dr = \sin h \left\{ 1 - \frac{(4h)^2}{6 \cdot 10} + \frac{(4h)^4}{6 \cdot 10 \cdot 14 \cdot 18} - \dots \right\} \\ - \cos h \left\{ \frac{4h}{6} - \frac{(4h)^3}{6 \cdot 10 \cdot 14} + \dots \right\}. \quad (10)$$

Thus, when $h = \frac{1}{4}\pi$,

$$2 \int_0^1 \cos\left(\frac{1}{4}\pi r^4\right) r dr = \frac{1.32945}{\sqrt{2}}, \quad 2 \int_0^1 \sin\left(\frac{1}{4}\pi r^4\right) r dr = \frac{.35424}{\sqrt{2}},$$

$$[2 \int_0^1 \cos\left(\frac{1}{4}\pi r^4\right) r dr]^2 + [2 \int_0^1 \sin\left(\frac{1}{4}\pi r^4\right) r dr]^2 = .9464.$$

Again, when $h = \frac{1}{2}\pi$,

$$2 \int_0^1 \cos\left(\frac{1}{2}\pi r^4\right) r dr = .77989, \quad 2 \int_0^1 \sin\left(\frac{1}{4}\pi r^4\right) r dr = .43828,$$

$$[2 \int_0^1 \cos\left(\frac{1}{2}\pi r^4\right) r dr]^2 + [2 \int_0^1 \sin\left(\frac{1}{2}\pi r^4\right) r dr]^2 = .8003.$$

Again, when $h = \pi$,

$$2 \int_0^1 \cos(\pi r^4) r dr = .3740, \quad 2 \int_0^1 \sin(\pi r^4) r dr = .5048,$$

$$[2 \int_0^1 \cos(\pi r^4) r dr]^2 + [2 \int_0^1 \sin(\pi r^4) r dr]^2 = .3947.$$

Hence in this case, as in the preceding, we may consider that aberration begins to be decidedly prejudicial when the wave-surface deviates from its proper place by about a quarter of a wave-length.

As an application of this result, let us investigate what amount of temperature-disturbance in the tube of a telescope may be expected to impair definition. According to the experiments of Biot and Arago, the refractive index μ for air at

temperature t° C. and at atmospheric pressure is given by

$$\mu - 1 = \frac{.00029}{1 + .0037t}.$$

If we take the freezing-point as standard temperature,

$$\delta\mu = -1.1t \times 10^{-6}. \quad . \quad . \quad . \quad . \quad (11)$$

Thus, supposing that the irregularity of temperature t extends through a length l , and produces a retardation of a quarter of a wave-length,

$$\frac{1}{4}\lambda = 1.1lt \times 10^{-6};$$

or, if we take $\lambda = 5.3 \times 10^{-5}$,

$$lt = 12, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

the unit of length being the centimetre.

We may infer that, in the case of a telescope-tube 12 centimetres long, a stratum of air heated one degree Cent., lying along the top of the tube and occupying a moderate fraction of the whole volume, would produce a not insensible effect. If the change of temperature progressed uniformly from one side of the tube to the other, the result would be a lateral displacement of the image without loss of definition; but in general both effects would be observable. In longer tubes a similar disturbance would be caused by a proportionally less difference of temperature.

In the ordinary investigations of the aberration of optical instruments attention is usually given to a quantity called the *longitudinal* aberration, which is the distance between the geometrical focus and the point at which the extreme ray meets the axis. In order to adapt these calculations to our purpose, it is necessary to establish the connexion between longitudinal aberration and the deviation of the actual surface of the converging waves from a truly spherical surface having its centre at the geometrical focus.

If the axis of symmetry be taken as that of z , and the tangent plane to the wave-surface as plane of xy , we have as the equation of the ideal wave-surface,

$$(z-f)^2 + x^2 + y^2 = f^2,$$

f being the distance of the focus from the origin; or if we limit our attention to the plane $y=0$,

$$z = f - \sqrt{f^2 - x^2} = \frac{1}{2} \frac{x^2}{f} + \frac{1}{8} \frac{x^4}{f^3}. \quad . \quad . \quad . \quad . \quad (13)$$

The actual wave-surface, having at the origin the same curvature, is represented by

$$z' = \frac{1}{2} \frac{x^2}{f} + \kappa \frac{x^4}{f^3}, \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where κ is a constant depending upon the amount of aber-

ration. The distance (h) between the surfaces is given by

$$h = z - z' = \left(\frac{1}{8} - \kappa\right) \frac{x^4}{f^3}. \quad \dots \quad (15)$$

The equation to the normal to (12) at the point z' , x is

$$\frac{\zeta - z'}{-1} = \frac{\xi - x}{\frac{x}{f} + \frac{4\kappa x^3}{f^3}};$$

so that when $\xi = 0$,

$$\zeta = z' + \frac{f}{1 + \frac{4\kappa x^2}{f^2}} = f + \frac{1}{2} \frac{x^2}{f} (1 - 8\kappa) + \dots$$

If the longitudinal aberration be called δf ,

$$\delta f = \zeta - f = \frac{1}{2} \frac{x^2}{f} (1 - 8\kappa). \quad \dots \quad (16)$$

Thus by (13) and (14),

$$\frac{h}{\delta f} = \frac{x^2}{4f^2} = \frac{1}{4} \alpha^2, \quad \dots \quad (17)$$

where α denotes the angular semi-aperture. Taking the greatest admissible value of h as equal to $\frac{1}{4}\lambda$, we shall see that δf must not exceed the value given by

$$\delta f = \lambda \alpha^{-2}. \quad \dots \quad (18)$$

As a practical example, we may take the case of a single lens of glass collecting parallel rays to a focus. With the most favourable curvatures the longitudinal aberration is about $f\alpha^2$; so that α^4 must not exceed $\lambda \div f$. For a lens of 3 feet focus, this condition is satisfied if the aperture do not exceed 2 inches. In spectroscopic work the chromatic aberration of single lenses does not come into play, and there is nothing to forbid their employment if the above-mentioned restriction be observed. I have been in the habit of using a plano-convex lens of plate-glass, the curved side being turned towards the parallel light, and have found its performance quite satisfactory. The fact that with a given focal length the extreme error of phase varies as the fourth power of the aperture is quite in accordance with practical experience; for it is well known that the difficulty of making object-glasses for telescopes increases very rapidly with the angular aperture.

When parallel rays fall directly upon a spherical mirror, the longitudinal aberration is only one eighth as great as for the most favourably shaped lens of equal focal length and aperture. Hence a spherical mirror of 3 feet focus might have an aperture of $2\frac{1}{2}$ inches, and the image would not suffer materially from aberration.

[To be continued.]

XLVII. *On the Conjugate Positions of two Circular Coils of Wire.* By W. GRANT, Assistant in the Physical Laboratory, University College, London*.

[Plate XII.]

WHILE recently engaged on some experiments on induction, I observed certain circumstances which I had not before noticed, and which seemed deserving of further attention. I was therefore led to inquire a little more closely into these matters; and although the investigation is by no means full or complete, I have obtained one or two results which I thought I might venture to lay before the Physical Society. The apparatus used in these experiments consisted, as at first arranged, of two coils of copper wire, one of which was connected in circuit with a battery of three Leclanché cells, and with a microphone which was actuated by a watch, while the other was connected with a telephone, in order that the induced currents, while passing through it, might render audible the beating of the watch which was used as the source of sound.

A modification of this arrangement was afterwards tried, a Grove's battery of twelve cells being substituted for the Leclanché battery, and a key being used for making and breaking the circuit. This was done in order to obtain greater inductive effects between the coils than could be obtained from the variations in the strength of the current which were caused by the action of the microphone. It was found, however, that with a little care in the adjustment of the coils, one cell gave sensibly as great an effect in the telephone as twelve cells; in subsequent experiments, therefore, the Grove's battery was discarded, and that of Leclanché again resorted to.

Now if two similar coils, connected as above described, are arranged with their planes parallel and their axes coincident, it is found that they may be separated to a considerable distance before the sounds which are heard in the telephone on making and breaking the circuit are obliterated. But it is also found that if the planes of the coils are kept parallel, the one in connexion with the telephone (that is, the secondary coil) may be placed in certain positions in the neighbourhood of the primary coil, and even in contact with it, without sounds being heard in the telephone. This happens when the mutual inductive effect between the two coils becomes zero; and when they are so placed as to fulfil this condition, they are said to occupy conjugate positions relatively to each other.

* Communicated by the Physical Society, having been read June 28th, 1879.

With the first arrangement of apparatus it was possible to place the coils so as to get complete silence in the telephone. With the powerful current from twelve Grove's cells and the key for making and breaking the circuit, however, the silence is not absolute; but in the positions which give a minimum of sound the sound is very faint, being just audible and no more. This faint sound may perhaps be accounted for partly because the different convolutions of wire in the secondary coil experience slightly different inductive effects from the primary one, and partly because it is difficult to adjust the positions of the coils with any great degree of accuracy without having special arrangements for the purpose.

Now it was found that the various conjugate positions in which the secondary coil could be placed in the neighbourhood of the primary one were situated in a path along which it could be moved either towards or away from the primary coil without sounds being heard in the telephone, but that with a slight deviation from this path to either side the sounds were again heard.

In order to ascertain whether the direction of the currents in the secondary circuit was reversed when the coil was moved from one side of the path to the other, a delicate reflecting galvanometer was substituted for the telephone, and the position of the coil so adjusted that on making and breaking the circuit no deflection of the galvanometer was observed. The coil was now moved slightly away from this position, say, towards the right; and the direction of the deflection of the galvanometer on making contact was noted, that on breaking being, of course, in the opposite direction. The coil was now moved towards the left to the other side of the path, and the direction of the deflections again observed; and it was found that they were now reversed. We may therefore infer that this path (which, if it could be fully traced, would of course constitute a surface of revolution about the axis of the primary coil) divides space into two regions, in one of which the inductive action of the primary coil has the opposite direction to what it has in the other.

This path appeared to be slightly curved; and it seemed as if a part of it might very readily be traced. The part which appeared to be best suited for this purpose was that along which the secondary coil has to pass while being moved away from contact with the primary one parallel to it to a position at some distance from it, as here the inductive effect is greatest, and therefore any deviation of the coil from the proper position in the path is most easily detected. As the coils are further separated, however, the position of the path becomes more difficult to trace, until at last we lose it altogether.

In order, then, to trace a curve which would represent this path, it was necessary to find several points in it whose positions could afterwards be accurately laid down. This was done by fixing the secondary coil in several positions successively and determining the position of a certain point in it with relation to certain fixed objects, by measurements which were afterwards used as abscissæ and ordinates in tracing the curve. These measurements were taken in inches; and their values are given in the annexed Table, where the columns headed x and y are those of abscissæ and ordinates respectively.

x .	y .	x .	y .
0.625	4.0	4.5	7.5
1.0	4.19	5.0	8.12
1.5	4.56	5.5	8.75
2.0	5.0	6.0	9.37
2.5	5.5	6.5	10.0
3.0	5.94	7.0	10.62
3.5	6.44	7.5	11.25
4.0	7.0	8.0	11.87

No special arrangement was used to adjust the parallelism of the coils, and only one measurement was taken for each number; hence the irregularity in the increase of the ordinates. The point whose position was determined in each case was the centre of the plane of the secondary coil; and that is the point which is situated in the curve when silence is maintained in the telephone.

The curve (Pl. XII.) is that found in this way; and it represents the path which the selected point of the coil has to follow in order that silence may be maintained in the telephone. C_1 and C_2 are sections of the primary and secondary coils respectively; C'_2 and C''_2 represent the secondary coil in two other conjugate positions. The lines A, A and P, P represent the axis and plane of the primary coil. The points a, b, c , &c. are the intersections of the abscissæ and ordinates, and represent the successive positions occupied by the selected point of the secondary coil when the measurements were taken by means of which the curve was traced. As the coils became further separated, however, the position of the curve became less distinct; and so no attempt was made to trace it further.

If, now, we suppose the curve to rotate round the axis of the primary coil, a surface will be generated of which it is a section; and if we observe the conditions necessary for placing the secondary coil in the curve in the proper position for silence, we may place it in any part of the surface with a like result.

The reason why we are enabled to trace a curve in this way

will be found by referring to the lines of force due to a circular current. These lines are represented by closed curves surrounding the section of the wire through which the current flows ; and they are given in Prof. Clerk Maxwell's work '*On Electricity and Magnetism*,' vol. ii. pl. 18. If we draw tangents to them parallel to the plane of the circular current, it will be found that the points where they touch are situated in a curve somewhat similar to that which we have found by experiment. The two curves, however, will not be found to coincide exactly, except in the case where the secondary coil does not enclose a space—that is to say, when its diameter is infinitely small. With respect to the curve drawn through the points of contact of the tangents to the lines of force, it will be seen that the direction of all these lines between the curve and the axis of the circular current is away from, and that their direction on the other side of the curve is towards the plane of the circular current : hence on opposite sides of the curve their tendency is to produce currents in opposite directions.

If the curve is now supposed to revolve round the axis of the circular current, all lines of force enclosed by the surface generated will tend to produce currents in one direction, while all lines outside the surface will tend to produce currents in the opposite direction. Therefore, when the secondary coil is so situated with respect to this surface that as many lines of force pass through it in one direction as in the other, the resultant inductive effect on it will be zero ; and this will be the case when it occupies any of the conjugate positions*.

It is evident from this, therefore, that we may combine the coils in several ways for the suppression of inductive effects :—first, by placing them close together face to face with their axes coincident, and so arranged that one of them may be moved across the face of the other parallel to their planes till a balance is obtained ; secondly, by placing them at some distance apart with their planes parallel and their axes coincident, and so arranged that if their planes are vertical each of them may be made to rotate round its vertical diameter ; then if they are joined together when their axes are coincident, and combined like parallel rulers, they may be made to rotate together until a balance is obtained. With regard to this

* In what precedes, the planes of the coils have been always supposed to be parallel to each other ; but it evidently follows from the reasoning here indicated that, if any set of parallel tangents be drawn to the lines of force and a curve be traced through the points of contact, an infinitely small coil would experience no inductive effect if it were placed with its centre anywhere in this curve, and with its plane parallel to the given set of tangents.

combination, it may be observed that the greatest inductive effect occurs when the planes of the coils are at the greatest distance from one another—and that as the planes approach, this effect gradually diminishes, until, when they still are at some distance, it becomes nothing.

Another, and perhaps more convenient, way of combining them is to place them, as in the last case, with their planes parallel and their axes coincident, the distance between them being equal to, or a little greater than, the radius of either coil : then, if their planes are vertical, we may fix one of them in that position ; and if the other is capable of rotating round its vertical diameter, it will be found that when it has rotated through 90° (that is, when the planes of the coils are at right angles) the inductive effects in the secondary circuit have ceased. If the coil is made to rotate through a few degrees to one side of this position, the currents induced in it will be in a certain direction ; and if it is rotated to the other side, their direction will be found to be reversed.

As with either of these combinations we could pass from sound to silence, some experiments were made in order to compare the rate of diminution of the induced currents with the movements of the coils in passing from a maximum to a minimum of inductive effects.

For this purpose the coils were placed with their faces in contact and their axes coincident, the secondary one being joined in circuit with a reflecting galvanometer. In this position five observations were taken and the mean recorded. They were now separated until their planes were an inch apart, and a mean of five observations again taken ; and this process was repeated at intervals of half an inch till the distance between them was increased to five inches.

They were now arranged as in the first combination, their faces being in contact during all the experiments ; and while their axes were coincident, five observations were taken and the mean recorded. One of them was now slid over the other, the faces being still in contact, through a distance of half an inch and a mean recorded as before ; and this process was repeated at intervals of half an inch till a balance was established.

The second and third combinations were treated in the same manner, the coils being moved by steps of 10° at a time, and readings taken till a balance was obtained ; and as the deflections were small in all cases, they were taken as being proportional to the strength of the currents.

The numbers given in the annexed Table are those found in the way indicated, the mean in each case being that of five experiments.

Axes of coils coincident.		First combination.		Second combination.		Third combination.	
Distance of planes.	Mean.	Distance of axes.	Mean.	Angle.	Mean.	Angle.	Mean.
0.625	88	0	88	0	8	0	17
1.0	77	0.5	84	10	7	10	17
1.5	52	1.0	70	20	7	20	17
2.0	38	1.5	57	30	6	30	16
2.5	28	2.0	40.6	40	5	40	16
3.0	22.5	2.5	30.8	50	4	50	15
3.5	17	3.0	20	60	3	60	12
4.0	13.4	3.5	10	65	0	70	9
4.5	10	4.0	0	...	...	80	5
5.0	8	...	...	...	...	85	2
						90	0

Note.—I may state here that I intend to continue this subject, and, when time permits, to trace some of the curves of equal induction.

XLVIII. *Notices respecting New Books.*

Instructions for testing Telegraph-Lines and the technical Arrangement of Offices. Originally written on behalf of the Government of India, under the Orders of the Director-General of Telegraphs in India. By LOUIS SCHWENDLER. Vol. I. Second Edition, Authorized by H. M. Secretary of State for India in Council. London: Trübner and Co., Ludgate Hill. 1878. (8vo, pp. xii and 239.)

THE title-page contains sufficient information as to the sanction under which this volume has been composed and published. Its contents may be enumerated as follows. It consists of two parts, each subdivided into two sections. The first part treats of "Line-testing Arrangements," its two sections being devoted to a description and an exposition of the mathematical theory of (1) Wheatstone's Bridge, (2) the Differential Galvanometer, considered as means of accurately comparing electrical resistances, and so of testing lines. For one of these means, however, the Author has a decided preference: he considers Wheatstone's Bridge "by far the best: it combines simplicity of arrangement with great accuracy and sufficient sensibility through wide ranges." The second part treats of "Line-testing," its two sections being devoted to (1) Regular Tests, *i. e.* Conduction Tests and Insulation Tests; (2) Fault-testing, the faults being those of Conduction, Insulation, and Interference of Circuits, or "Disconnections," "Earths," and "Contacts." These sections are supplemented by Appendices, fifteen in number, which form more than a third of the volume;

some are devoted to developing the theory on which the discussions in the previous section are based, others to explaining special applications of those sections. Thus the first Appendix to the second section of the second part is devoted to the theory of finding the "Resultant Fault," while the second Appendix, "on the execution of Regular Tests," lays down general and practical rules for the operation, and gives an account of the routine observed in their execution on the Indian lines.

Considered as an account of the principles on which the methods are based, the work can hardly be regarded as in all respects satisfactory: if this had been the Author's sole object it might be fairly said that the arrangement is inconvenient and the exposition obscure. For instance, the accounts of Ohm's law and Kirchhoff's corollaries, which are the foundation of the theory of Wheatstone's Bridge, are the subject of two Appendices; and the connexion between them and the various parts of the theory laid down in the preceding section is not very clearly made out. In fact, we doubt whether more than a very few readers would actually make it out unless they had recourse to other books. Added to this, it may be noted that the correction of the proof-sheets does not seem to have been very thoroughly made*.

This, however, is a matter of comparatively small importance; for doubtless the work will be quite intelligible to the class of readers for whom it is written, namely those who are practically concerned in working a Telegraphic System. The information on all points in which such readers will be mainly interested seems to be adequate and to be illustrated by a sufficiency of examples, some of which are very striking. For example (p. 183), "On a single line 420 miles in length . . . there occurs imperfect communication due to a partial Earth." From the data the position of the fault is calculated to be about 363 miles distant; and on examination the wire is found to be off two posts and resting on the ground at $362\frac{1}{2}$ miles from the testing-station. Again (p. 203) "Between two lines each 300 miles in length occurs a contact:" measurements and calculation give about 56 miles for the distance of the contact; and accordingly at about the 56th mile a dead snake is found twisted round the wires, "which during the rain caused considerable cross leakage."

The Author's object in the composition of the work is best stated in his own words:—"During my stay in Europe I have had many opportunities to watch the working of other Telegraph Administrations, and have been surprised to find how little progress Testing has made. I was always told, 'Yes, we should like to introduce a general system of Testing; we know its great practical utility; but show us a system to do it, and which will work satisfactorily.'

"Here is a book which I believe contains such a system; and although I am well aware of many defects, I know it has done good service in India, and by it the objects in view have been

* For example, the letters O and o are repeatedly printed for 0.

obtained, *i. e.* we know quantitatively the electrical state of the lines at all hours of the day, and seasons of the year; we are able to localize all faults of all kinds very accurately, and repair them with dispatch; we test all our telegraphic material, and by it have greatly improved its essential qualities; we are not groping in the dark any more—we *measure and know.*”

XLIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. vii. p. 442.]

May 14, 1879.—Prof. P. M. Duncan, M.B., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. “Further Observations on the Pre-Cambrian Rocks of Caernarvon.” By Prof. T. M’Kenny Hughes, M.A., F.G.S.

The author divides these into (1) the volcanic series, (2) the felsitic series, (3) the granitoid series. He traces the former of these, consisting of coarser and finer varieties, from Caernarvon to near Port Dinorwig. Beyond these come the felsite series, which is overlapped by grits and conglomerates as far as the Bangor road, N.E. of Brithdir. Above the latter comes the “volcanic series,” well developed in the neighbourhood of Bangor. The author is of opinion that the Cambrian conglomerate, with associated grits, may be traced in the edge of the older massif from Twt Hill, Caernarvon, to Garth Point, Bangor, and that the beds in each of these places and near Brithdir, recently described as separate, are identical; also that the bed with purple fragments near Tairffynnon and the Bangor poorhouse are only Cambrian conglomerate faulted down. Further, he considers that the strata of the above three series are fairly parallel throughout, and that they only form three subdivisions of one great series.

2. “Notes on the Structure of the Palæozoic Districts of West Somerset.” By A. Champernowne, Esq., F.G.S., and W. A. E. Ussher, Esq., F.G.S.

The authors confirmed the general accuracy of Mr. Etheridge’s views as to the structure of North Devon and West Somerset, but differed from him in ascribing the limestone of Cannington Park to the Carboniferous, both on account of lithological character, the fossils in Taunton Museum said to be obtained from it, and the latitude of its position with reference to the Carboniferous Limestone of the Mendip, South Wales, and the steep and flat Holmes. They described four traverses made by them in West Somerset. 1st. From Dulverton to Dunster, in which, proceeding northwards, the following beds were encountered:—Culm-measures faulted against Pilton Beds (Upper Devonian); Pilton Beds faulted against Pickwell-Down Sandstone (base of Upper Devonian); Pickwell-Down Sandstones becoming slaty in passing into Morte slates

(Middle Devonian) and troughed in them by faulted synclines; Morte slates passing into Ilfracombe slates (overlying Hangman grits) near Cutcombe; Hangman grits evidently faulted against Foreland grits, as no representative of the Lynton beds is present between Oaktrow and Timberscombe.

In traverse 2, the fault between the Hangman and Foreland grits is proved by the presence of the Lynton beds in the valley west of Luccot Hill and their conformable infraposition to the Hangman series, and abrupt termination by fault against the Foreland grits of Porlock and Oare Hills. At Oare a patch of schist of the Lynton zone was noticed resting on the Foreland grits on the north side of the fault.

The 3rd traverse, in the Tone valley, gave the following succession of beds:—Culm-measures on Pilton beds; Pilton beds with grits, much flexured, on Olive slates with *Lingula* and grits with *Cucullæa*, conformably overlying Pickwell-Down grits, which make a conformable junction (following the feature) with the underlying quartziferous slates of the Morte series (Middle Devonian); the latter were observed between Huish Champflower and Clatworthy; but as the Middle Devonian slates appear to extend considerably northward in the Brendons, they were not traversed beyond Clatworthy.

The 4th traverse, from West Quantockshead to Cannington Park, proved the composition of the Quantocks along that line to be grits, in places associated with schistose shales, apparently belonging to the Hangman series (Middle Devonian); whilst the Palæozoic inliers, in the Triassic area of Bridgewater, are unlike the Quantock rocks in character. The limestones of Asholt and Hollwell, associated with slates of the Ilfracombe series, are very similar to varieties of the South-Devon limestone, and are quite unlike the limestone of Cannington Park.

3. "The Whin Sill of Teesdale as an Assimilator of the surrounding Beds." By C. T. Clough, Esq., F.G.S.

Owing to the general absence of mechanical disturbance, the author is of opinion that "the whin consists in part of altered sedimentary beds, that it partly represents beds which were once in the position it now occupies, that it did not make room for itself simply by thrusting aside these beds, but also by incorporating them into itself." He proceeds to describe sections at Caldron Snout, Cronkley Fell, Noon Hill, &c., which seem to him inexplicable on any other theory. The author discusses objections on chemical grounds, holding that the general uniformity in chemical composition of the Whin may be explained by supposing the absorbed beds to have permeated a large mass of the Whin, as an alloy does melted metal. He thinks the explanation may be extended to other intrusive masses.

4. "On the Silurian Rocks of the Valley of the Clwyd." By Prof. T. M'Kenny Hughes, M.A., F.G.S.

The author gives a preliminary sketch of the Silurian rocks of the southern and western part of the Clwyd valley. He describes

first some beds below the horizon of the Denbigh Grits at Ffriddfawr which agree very well in their characters with the base of the Coniston Grit, and others near agreeing with the passage-beds between these Grits and Flags. He next describes sandstones in the Clywedog valley, the equivalents of the lower Grits, and, lastly, at Bod Renail, flags &c., the Pale Slates, which contain Graptolites and are thus to be identified with the Graptolitic mudstones of the Lake-district. Thus he is of opinion there is a basement-series here for the Silurian, corresponding in all its details with that in the Lake-district.

L. Intelligence and Miscellaneous Articles.

ON THE ALTERATION OF THE DENSITY OF STEEL BY HARDENING AND TEMPERING. BY CARL FROMME.

M. BARUS* has communicated some experiments on the thermoelectric place and the electric conductivity of hardened steel. A comparison of the "thermoelectric hardness" of steel wires hardened at different degrees of heating with the measurements of the specific gravity made on hardened and tempered steel rods and published by me† showed a good accordance between the numbers in the two cases. As there only a very brief survey of the results obtained, so far as they appeared necessary for the establishment of an opinion advanced respecting magnetic phenomena, was given, but measurements of this sort have never, to my knowledge, been communicated, I hold it not superfluous to give here the more precise data furnished by the observations.

Four rods were cut from the same larger rod, each 100 millims. long and 7 millims. thick. Table I. contains the specific gravities of the rods, first in the state in which they were procured from the maker (crude), then in the glass-hard state (hardened in cold water), tempered yellow, tempered blue, and grey.

TABLE I.

	Rod 1.	Rod 2.	Rod 3.	Rod 4.
Crude.....	7.8154	7.8188	7.8128	7.8200
Hardened	7.7420	7.7565	7.7687	7.7604
Tempered yellow	7.7789	7.7918	7.7962	7.7922
„ blue	7.7942	7.8002	7.8054	7.7994
Grey	7.8174	7.8227	7.8192	7.8209

Putting the volume of the rod in the crude state equal to 1, we get:—

* Wied. *Ann.* vii. pp. 383-414 (1879).

† *Nachr. d. k. Ges. Wiss. zu Göttingen*, 1876, No. 7, p. 165.

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TABLE II.

	Rod 1.	Rod 2.	Rod 3.	Rod 4.
Crude.....	1·0000	1·0000	1·0000	1·0000
Hardened	1·0095	1·0080	1·0057	1·0077
Tempered yellow	1·0047	1·0035	1·0021	1·0036
„ blue	1·0027	1·0024	1·0010	1·0026
Grey	0·9997	0·9995	1·9992	1·9999

Six rods, each two of which were cut from the same rod. The length amounts to 100 millims.; the thicknesses 4·2, 2·65, and 2·55 millims. respectively. In Table III. are added also the specific gravities assumed by the rods when (after the conclusion of the other measurements) submitted to a bright red heat and afterwards very slowly cooled.

TABLE III.

	4·2 millims. thick.		2·65 millims. thick.		2·55 millims. thick.	
	Rod 1.	Rod 2.	Rod 1.	Rod 2.	Rod 1.	Rod 2.
Crude.....	7·7994	7·8066	7·7642	7·7568	7·7813	7·7757
Hardened	7·7218	7·7290	7·6669	7·6570	7·6847	7·6862
Tempered yellow.	7·7589	7·7680	7·7124	7·7069	7·7367	7·7250
„ blue ...	7·7687	7·7719	7·7367	7·7274	7·7667	7·7584
Grey	7·8015	7·7952	7·7599	7·7532	7·7828	7·7852
Heated to bright redness	7·7936	7·7859	7·7510	7·7382	7·7512	7·7534

Again, if we put the volume of a rod in the crude state = 1, we get:—

TABLE IV.

	4·2 millims. thick.		2·65 millims. thick.		2·55 millims. thick.	
	Rod 1.	Rod 2.	Rod 1.	Rod 2.	Rod 1.	Rod 2.
Crude.....	1·0000	1·0000	1·0000	1·0009	1·0000	1·0000
Hardened	1·0100	1·0100	1·0127	1·0130	1·0126	1·0116
Tempered yellow.	1·0052	1·0047	1·0067	1·0065	1·0058	1·0066
„ blue ...	1·0040	1·0045	1·0036	1·0038	1·0019	1·0022
Grey	0·9997	1·0015	1·0006	1·0005	0·9998	0·9988
Heated to bright redness.....	1·0008	1·0027	1·0017	1·0026	1·0039	1·0029

The mean values of the volumes are accordingly:—

TABLE V.

Rods.	7 millims. thick.	4·2 millims. thick.	2·65 millims. thick.	2·55 millims. thick.
Crude.....	1·00000	1·00000	1·00000	1·00000
Hardened	1·00772	1·01000	1·01285	1·01210
Tempered yellow	1·00347	1·00495	1·00660	1·00620
„ blue	1·00217	1·00425	1·00370	1·00205
Grey	0·99957	1·00060	1·00055	0·99930
Heated to bright redness		1·00175	1·00215	1·00340

It follows, from the above observations, that—

1. On the hardening of steel there ensues a diminution of its specific gravity—that is, an increase of its volume.

2. The thicker the rod the less is it hardened; that is, the smaller is the increment of its volume, this being reckoned from the volume of the crude rod.

3. By tempering, the specific gravity is again increased, consequently the volume diminished; so that when the tempering-colour is yellow the increment of volume is reduced to one half its previous amount. This law holds rigorously with rods of all thicknesses; the deviations lie quite within the limits of errors of observation.

4. If the blue colour of oxidation be reached, the volume undergoes a further diminution, in the mean about one fifth of the previous increment. The less accordance in this case between the observations made on different rods of the same thickness probably arises from the greater difficulty of distinguishing between the darker hues of oxidation.

5. It was only with the disappearance of all colours (in the grey state) that the rods resumed the specific gravity and volume which they possessed before the hardening—that is, in the crude state in which they were procured. That, notwithstanding this, the two states, grey and crude, are absolutely different follows (as is shown *loc. cit.*) from the fact that a grey rod takes up much more permanent magnetism than a crude one (in an observed case more than twice as much).

6. If thereupon the rods are heated to bright redness and let cool very slowly, the specific gravity again diminishes, or the volume increases. This increment amounted on the average to somewhat more than one sixth of that produced in the crude rod by quenching.

I wish, in conclusion, to call attention to the interesting question, How, in a hardened steel rod, does the density vary inward from the periphery to the axis? The supposition that first presents itself, and which I made *loc. cit.* p. 165 *ad fin.*, is that the rod may be imagined as consisting of concentric layers whose hardness diminishes rapidly from the outside inwards, finally enclosing a uniformly soft core. I have in the meantime endeavoured to test this view by the following observations:—

The 7-millim.-thick rod 1, which at last (in the grey state) had shown the specific gravity 7·8174, was hardened in cold water. The sp. gr. then obtained was 7·7417. I now placed the rod in dilute hydrochloric acid, and during a long period I daily determined its specific gravity Δ .

During the first nine days it sank, in an absolutely regular manner, to $\Delta=7\cdot7130$. The weight of the layer that had been eaten away amounted to only 0·704 gram out of 29·746 grams, the original weight of the rod. The mean specific gravity δ of the removed layer was calculated $=10\cdot5$. During the following seven days Δ rose again, just as regularly, to 7·7412—thus nearly to the initial value. The layer removed by the acid was $m=1\cdot703$ gram, and its calculated mean specific gravity $\delta=7\cdot3$.

During the next four days ($m=1.014$ gram) Δ sank to 7.712, and then rose again.

Meanwhile some cracks in the interior of the rod had come to light; so that I broke off the observations; but the results of this one series are so remarkable that a further investigation of the phenomenon is desirable.—Wiedemann's *Annalen*, 1879, No. 10, (vol. viii.) pp. 352–356.

DETERMINATIONS OF THE VAPOUR-DENSITIES, AT HIGH TEMPERATURES, OF SUBSTANCES THAT ATTACK MERCURY. BY L. PFAUNDLER*.

In connexion with the numerous newer methods for determining vapour-densities, especially the most recent, by V. Meyer and C. Meyer (*Beibl.* iii. p. 252), Pfaundler reminds us of one mentioned already in 1870, in the *Berichte des naturwissenschaftlich-medicinischen Vereins in Innsbruck*. The construction of the apparatus he employed was substantially that of a Regnault air-thermometer; only, instead of the simple glass cylinder, a glass vessel formed of several compartments one above another, and connected by capillary tubes, served for holding the vapour, by which the vapour to be investigated could be kept separate from the mercury, at least during the observation. Moreover, in the case of dissociation, diffusion of the constituents in unequal quantity can be prevented by employing a capillary tube coiled in any manner instead of the second, third, &c. compartments. The temperature is observed on an exactly like-constructed air-thermometer; and the difference of pressure in the latter and in the vapour-apparatus gives the pressure of the vapour very accurately, since many errors eliminate one another. The substance to be examined is introduced through a narrow tube into the apparatus, which is then hermetically closed, the capacity of the vessels having been previously determined in the known volumimetric manner. In the method used by V. and C. Meyer also, the air (nitrogen &c. respectively) displaced by the vapour prevents contact with the barrier-liquid; the difference of principle between the two methods, however, is this—that in the Meyers' the volume of the vapour is deduced from that of the displaced air, while with Pfaundler the determination has in view the measurement of the pressure of the vapour at constant volume. Pfaundler himself regards the Meyerian method as by far the simpler, while he considers his own the more exact and hence more suitable for testing theoretical relations.—RTH. in the *Beiblätter zu den Annalen der Physik und Chemie*, 1879, No. 7, p. 484–5.

* *Chem. Ber.* xii. pp. 165–169 (1879).

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

DECEMBER 1879.

LI. *A new Explanation of the Colour of the Sky.*
By E. L. NICHOLS, *Ph.D.*, *Göttingen* *.

IN seeking an explanation of the colour of the sky, there is little use in looking back beyond Newton. Other philosophers † had discussed the question; but their reasoning was not such as, in the present state of science, demands our earnest attention. Newton's opinion stands to this day as near the truth perhaps as any later hypothesis†:—

“The blue of the first order, though very faint and little, may possibly be the colour of some substances; and particularly the azure colour of the skies seems to be of this order; for all vapours when they begin to condense and coalesce in small parcels, become first of that bigness whereby such an azure must be reflected, before they can form clouds of other

* Communicated by the Author.

† Forbes, “On the Colours of the Atmosphere,” ‘*Edinburgh Trans.*’ 1840, reviews in an interesting manner the older authorities on this subject. Of these, Leonardo da Vinci, *Traité de la Peinture*, considered the blueness of the sky as simply the effect upon the eye of looking into the infinity of space. Mariotte, *Œuvres*, i. 229, insisted upon an actual blue colour of the air; and Euler (1762) said, “It is more probable that all the particles of the air should have a faintly bluish cast, but so very faint as to be imperceptible, until presented in a prodigious mass.” Muncke, on the contrary, declared the colour of the heavens to be an optical illusion, something purely subjective (see Gehler's *Wörterbuch*, article *Atmosphäre*). Even Sir David Brewster seems to have had doubts as to the reality of the sky's blueness.

† Newton, ‘*Optics*,’ Book II.

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colours. And so, this being the first colour which vapours begin to reflect, it ought to be the colour of the finest and most transparent skies, in which vapours are not arrived to that grossness requisite to reflect other colours—as we find it is by experience.”

This opinion, after nearly two hundred years, has been verified by Tyndall's * well-known experiment. Professor Tyndall in his memoir makes no reference to Newton's remarkable foreshadowing of his result, but comes independently to the conclusion that with his tubeful of finely divided matter, which, while its particles were still invisible under the microscope, appeared like a faint blue cloud, he has solved the mystery of the blueness of the heavens.

Several years before the publication of Prof. Tyndall's paper, Prof. Clausius †, of Bonn, starting with Newton's idea that the colour of the skies is the blue of the first order, investigated the conditions necessary to the production of such a colour. He shows that the reflecting medium cannot consist of minute solid drops, as he thinks Newton to have imagined, but must be composed of bubbles, the films of which do not exceed in thickness a quarter of the wave-length of violet light. Well aware, however, that in our atmosphere the particles of vapour assume all sizes up to that of the ordinary rain-drop, he goes on to prove that really blue skies occur under conditions of atmosphere admitting of finely divided vapour only, and that changes of pressure and temperature such as conduce to the formation of larger particles must at the same time produce in far greater numbers bubbles of the exceedingly small sizes necessary to the blue of the first order ; so that the general tint of the sky will pass into white rather than into any spectral colour.

How is Newton's opinion, supported by Prof. Tyndall's undoubted experimental evidence, to be reconciled with Clausius's contradictory result? The last-named physicist has shown conclusively that the light reflected by even the smallest particles of matter cannot be blue unless these particles are of vesicular structure. Tyndall has demonstrated the fact that such light is blue. It is the object of this paper to show the bearing upon this subject of a certain well-known fact—the dependence of the relative sensitiveness of the eye for different colours upon the intensity of the ray. It renders clear the nature of the above discrepancy, and affords foundation for a new hypothesis of the colour of the sky.

* Tyndall, 'Fragments of Science,' "On the Structure and Colour of the Skies."

† Clausius, Poggendorff's *Annalen*, lxxvi.

Even Helmholtz, to whom more than to any other we owe our knowledge of this fact, seems to have overlooked its application to the present question. On page 66 of the *Populäre wissenschaftliche Vorträge*, Heft 3, where the blueness of the sky is mentioned, he contents himself with Newton's explanation, illustrating the relative effects of coarsely and finely divided matter as follows:—

“The colour of the light reflected by the intervening particles depends largely upon their size. If a splinter of wood float upon the water and we make a little set of wave-rings near it by means of a falling drop of water, the waves will be reflected by the floating splinter as they would be by a stone wall. In the long ocean-swells, however, the little splinter would be tossed to and fro without noticeably impeding the progress of the waves. Now light, as we know, is also the wave-like progressive motion of a medium pervading the universe, and called the æther. The red and green rays have the longest waves, the violet and blue the shortest. Very fine particles, which disturb the homogeneity of the æther, must therefore reflect the latter rays more readily than they do the red and green light. In point of fact, the light reflected by such a medium is bluer the finer its particles, while coarser particles reflect all colours and give a white effect. Such is the blue of the heavens, that is to say of the atmosphere, seen against the blackness of open space. The purer and more transparent the air, the bluer are the skies.”

An attempt to decide how far this illustration really represents the process of atmospheric reflexion involves a discussion of the mechanical causes of reflexion in general. There are two hypotheses:—1st. That reflexion and refraction result from the action of the particles of the reflecting body upon the vibrating particles of æther. 2nd. That they arise from the difference between the density of the æther within the body and that within the adjacent medium. Fresnel (*Œuvres Complètes*, tome i., “Mémoire sur la Réflexion”), after long making use of the latter theory, rejected it for the first one, which seemed better applicable to the phenomena connected with Newton's rings.

Assuming such an action between the particles of the body and of the æther, it might be difficult to decide, in the case where the body is exceedingly small, for or against the possibility of a selective reflexion of the more refrangible rays. Since Fresnel's time this theory has been generally abandoned for the second one. A strong argument against it may be deduced from the refracting powers of gases. The various gases contain, by Avogadro's law, an equal number of mole-

cules per unit of volume ; in other words, the mass of the particles is in direct proportion to the density of the substance.

If reflexion and refraction result from impact between the particles of the gas and of the vibrating æther, or from equivalent interaction, we should expect to find, in the case of the different gases, that their power to reflect, and consequently (in accordance with the formulæ

$$R_p = \frac{\sin^2(i-r)}{\sin^2(i+r)}$$

for the reflected component polarized parallel to the plane of incidence, and

$$R_s = \frac{\tan^2(i-r)}{\tan^2(i+r)}$$

for the component polarized perpendicularly to that plane) their indices of refraction are in direct proportion to their density. How far this is from the fact appears from the following results of Dulong :—

Gas.	Density.	Refraction-index compared with air.
Air.....	1.000	1.000
O	1.1026	0.924
H	0.0685	0.470
N	0.976	1.020
Cl	2.470	2.623
CO	0.972	1.157
CO ₂	1.524	1.526
Cy	1.818	2.832
C ₂ H ₄	0.980	2.302
CH ₄	0.559	1.504

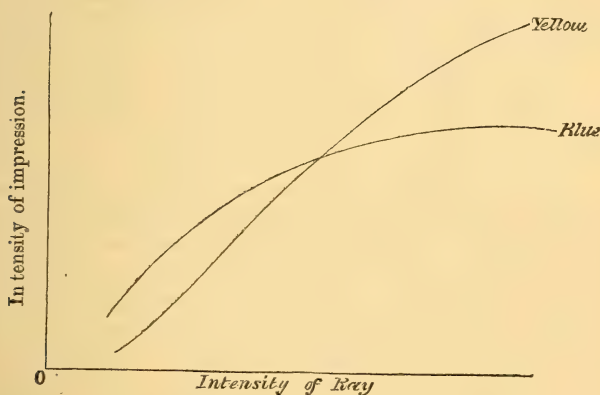
Under the second hypothesis, the analogy between the bit of wood tossed by water-waves and the process taking place when light is reflected vanishes completely. Any body, however large or small, which is capable of causing the æther pervading it to differ in density from the æther within the adjacent medium, must reflect and refract light, irrespective of the wave-length of the rays.

That peculiarity of the human eye the influence of which upon the theory of the colour of the sky it is proposed to discuss, is as follows :—

According to Young* and Helmholtz's theory of colour-impression, there are in the eye three sets of nerve-termini—one set chiefly influenced by the red, another by the green,

* Dr. Young, Works edited by Peacock, vol. i. p. 147.

the third by violet* rays. The impression of colour is the resultant of the intensities of these three effects. The impression produced by any kind of light is always the sum of the effects its components are capable of producing upon the three sets of nerve-ends. Helmholtz discovered that the impression upon these nerves is not directly proportional to the intensity of the ray, but that the "red"-nerves are subject to one law, and the "green" to another, while the "violet"-nerves have a distinct law of their own. For very feeble rays, the "violet"-nerves are very sensitive, while the "green" and "red"-nerves scarcely act at all†. As the intensity of the light increases, the "red" and "green"-nerves increase in activity, while the "violet"-nerves gradually become tired and dazzled, as it were, and so incapable of receiving their full share of the effect. For rays of dazzling brilliancy the "red"-nerves are in their most sensitive condition, while those affected by the more refrangible components of the ray are comparatively dull‡. The comparative effect of yellow and blue light at different intensities can be shown by curves (see figure). For feeble



intensities, the blue impression exceeds the yellow; the point soon comes where the curves cut one another; beyond this yellow exceeds the blue, and the latter seems to be approaching a maximum. Upon this peculiarity of the eye depend all

* The colours first chosen by Young were red, yellow, blue—probably because these were the commonly accepted "primaries." Later he adopted red, green, violet—a choice confirmed since by Helmholtz, Maxwell, &c.

† The expressions "red"-nerve, "green"-nerve, &c. are used here for the sake of brevity to denote the nerve-termini affected by those colours.

‡ Helmholtz, *Handbuch der physiologische Optik*, p. 319; see also Dobrowsky, Graef's *Archiv der Ophthalmologie*, Bd. xviii.

those striking changes of hue due to variations of the intensity of illumination. Of the simple colours, as the brightness of the ray increases, red and green change to yellow, *blue becomes white*. Besides these remarkable changes, familiar only to the spectroscopist, there are in the outdoor world a host of striking effects. Passing cloud-shadows on the water at noon-day reveal to us that what under shadow is indigo-blue becomes instantly, in the direct sunlight, a subtle silver-grey. The same cloud-shadows transform the sun-lit yellow-green of a neighbouring hillside into blue-green. All these effects are the necessary result of that peculiarity in the action of our nerve-termini. Of more direct interest at present is the influence upon white daylight. Such light at ordinary intensities affects the three sets of nerve-termini equally; the resultant impression is whiteness. Now daylight is simply the light of the sun weakened by manifold diffuse reflexions. The direct rays of the sun as we let them fall upon any colourless object appear also a white light; but on attempting at noon of a clear day to gaze into the sun's face, the impression is of blinding yellow. It is not that the direct rays differ in composition from diffuse daylight, but that the "violet"-nerves are ill adapted to transmit the action of such strong light. They shrink from their share of the work, bewildered and overcome, while the "green" and "red"-nerves are stimulated by the glare to extraordinary activity.

In nature the variety in the intensity of light is almost infinite. The moon with $\frac{1}{500000}$, perhaps, of the sun's illuminating-power seems bright; and she is in reality far brighter than the open sky. At noonday even she appears like a white cloud, around which the blue atmosphere often looks almost black by contrast. In passing from the intensity of the moon's rays to those reaching us from a corresponding bit of the open sky, we may perhaps take a step as great as that between the brightness of sun and moon. Already in the case of the moon's light the *yellow* impression given by direct sunlight has been supplanted by a blue one*. The contrast between the colour-impressions due to sunlight and to moonlight is far greater than we, in the lack of a means of direct

* This effect is of course frequently vitiated by the fact that the moon's rays on their way to us have to penetrate dense strata of aqueous vapour. Such vapour, as shown by Forbes ("On the Colour of Steam," Edinburgh Trans. 1843), absorbs all the more refrangible rays; and moonlight then appears golden, or even reddish. That *blue* is nevertheless the hue commonly associated in our minds with moonlight, the Venetian "moonlight" photographs (so called) bear witness. They owe their effect to the blue wash which converts a common daylight picture into the desired moonlight view.

comparison, are apt to realize; and yet the two kinds of light are identical in composition, varying from ordinary white daylight only in intensity.

In general, white light will seem bluer and bluer as its intensity diminishes, and this law will apply to the skies: as the light they reflect becomes fainter and fainter, they will increase in blueness, even though the light by the process of reflexion suffer no change in composition.

Nature offers some beautiful demonstrations of this fact*. Study the clouds at sunset at a time when the upper sky is full of fleecy cirri. Such clouds in a noonday light are of the purest white. Watch them now as the beams of the setting sun strike them horizontally. The western edges are of a brilliant white; the eastern portions are in deep shadow. Compare the lines of the various parts of the cloud—not forgetting that (different as they look), since the structure of the cloud does not vary, and since the light reflected from it is everywhere common daylight, the whole cloud should be, “objectively” speaking, of one colour, white. The entire shaded portion is nevertheless unmistakably blue; and the deeper the shadow, the more nearly it will approach the colour of the sky. On the sun-lit side the thinnest shreds of mist stand out bright and sharp, like snow-flakes against the blue void behind them. In the shadow, cloud and sky merge into one another, so nearly identical are their hues.

This phenomenon and that of the blueness of the sky are, then, cases of the effect of intensity of the light upon its *hue* (subjectively considered). Indeed they are the converse of the well-known change of blue light into white (see p. 430). A ray affecting at ordinary intensities the “violet” nerve-termini chiefly becomes capable, by a peculiarity of the eye, of giving, when bright enough, the red and green impressions also, and thus of appearing *white*. The same peculiarity of the eye makes a ray which is white when of ordinary brightness, seem blue as it becomes faint. In the latter case the

* Helmholtz gives as the reason why these effects come so rarely into notice, that the daylight which we unconsciously adopt as our standard changes in the same manner as the light we are observing, so that the effect is neutralized. “Dass das Sonnenlicht, welches wir bei Tage als das normale Weiss betrachten, selbst, bei verschiedener Lichtstärke, in ähnlicher Weise seine Farbe ändern muss wie die anderen” The appearance of blue light in deep narrow holes in snow-drifts is probably due to the same cause as the blue colour of the sky. As soon as such a hole is so deep that the interior is dark, the lower part is seen to be pervaded by a beautiful blue light. The snow affords a colourless screen, which, if the drift be large, is only penetrated by the merest traces of light. These, by virtue of their very small intensity, appear blue.

yellow impression simply dies away faster than the complementary *blue* one; and as the general intensity decreases, the ray seems bluer and bluer.

An interesting question is that concerning the nature of the medium in our atmosphere by means of which the light reaching us from the open sky has been reflected. Such light is polarized; and the angle of incidence at which its polarization is most complete corresponds to that which we should expect, were the refractive index of the reflecting-substance $=1^*$ (absolute index $=1.00029$).

A natural conclusion is that the atmosphere itself forms the reflecting medium, since common air has the refractive index 1.000294 . The common supposition seems nevertheless to be that the reflexion occurs at the surfaces of particles of water and of other foreign matter floating in the air. How this hypothesis is to be reconciled with the condition just stated, that the refractive index of the reflecting medium $=1$, it is not easy to imagine. Clausius, in accordance with his theory, supposes the light to be reflected from minute bubbles of water. The atmosphere, he argues, decreasing in density by imperceptible degrees upward, offers no break or discontinuity at which reflexion can occur. Nothing, I submit, can be further from the fact. Were our atmosphere visible, we should find it to be in a constant state of agitation, broken up into currents, vertical as well as horizontal, twisting and turning and eddying in futile efforts to reach a position of equilibrium. Differences of pressure and of temperature alike serve to cause this varied motion. Air warmed by contact with the soil in places exposed to the sun's rays rises to make room for cooler heavier masses; and so the horizontal currents which are commonly recognized as wind form but a small portion of the movements constantly taking place. The effect of such currents will be to form numerous surfaces at which discontinuities of density and of motion will occur. At each of these there will be reflexion†.

That mist, drops of water, and other foreign substances in the atmosphere also reflect light is an independent fact. The presence of such light in the sky is easily shown. It is pola-

* See Tyndall, article already cited.

† Whenever moist air and dry come into contact and the boundary between them is sharply defined, another cause of reflexion from the clear sky occurs. Ketteler, *Beobachtungen über die Farbenzerstreuung der Gase*, Bonn, 1865, shows that the refractive index of moist air is larger than when the air is dry.

rized at a different angle of incidence—that corresponding to the refractive index of water. It is chiefly reflected from clouds, and is in no way to be confounded with that reaching us from the clear sky.

Peekskill, New York,
July 23, 1879.

LII. *On the Separation and Estimation of Cadmium in the Presence of Zinc; with Remarks upon the Separation of Copper, Cadmium, and Zinc.* By CHRISTOPHER CLARKE HUTCHINSON, Assoc. R.C.Sc.I.*

A SHORT time ago I conducted an investigation on the influence of *mass* in chemical reactions, particularly directing my attention to the influence which copper, cadmium, and zinc exercise upon each other, and their mutual relations during precipitation. The two latter metals seem to have many points in common with each other, and show great similarity in their behaviour towards most chemical reagents.

The investigation necessitated an examination of the methods in use for the separation and estimation of these metals. Most of such processes proved very unsatisfactory; I was therefore obliged to make some experiments in order to determine the methods most reliable. In this paper I shall give the results I obtained, and the methods found most accurate. Standard solutions of the pure crystallized sulphates made from the pure metals were used throughout.

Separation of Cadmium and Zinc.—If an acid solution containing both of these metals be treated with sulphuretted hydrogen, the cadmium should be precipitated as the sulphide, while the zinc should remain in solution. The reaction, however, is not so complete as indicated; and its behaviour is dependent upon the amount of acid present in *excess*. If too great an amount of acid be present, the cadmium will only be imperfectly precipitated; while, on the other hand, if the solution be not sufficiently acid, a portion of the zinc undergoes precipitation as sulphide with the cadmium. A method of separation based upon this behaviour will only yield satisfactory results when the relative quantity of each metal present is approximately known, so that the quantity of acid can be adjusted (whether the one or the other be present in a larger quantity) so as not to prevent the total precipitation of cadmium or favour that of zinc. Even when very carefully conducted, it is often found necessary to redissolve the sulphides

* Communicated by the Author.

obtained, and to reprecipitate, so as to thoroughly eliminate every trace of zinc.

The method of separation described by Fresenius, and usually given by other authorities, is also open to the objection of not yielding concordant results, even though every precaution be observed in the process and the quantity of the reagents be carefully adjusted. This method is based upon the insolubility of the hydrated oxide of cadmium in an alkaline solution containing tartaric acid. To the hydrochloric acid solution of the two, as near as possible neutral, tartaric acid is added; sufficient sodium or potassium hydrate is then added until the solution is distinctly of an alkaline reaction. If this solution be boiled for about two hours, the hydrated oxide of cadmium separates out on cooling, the zinc being left in solution. After filtration, the hydrated oxide is ignited and weighed as the oxide (CdO). The zinc may be precipitated as the sulphide by ammonium sulphide, or titrated with sodium sulphide.

Two examples may be given, showing the mutual variability in estimations which should each have yielded the same result. In each case the determination of the amount of cadmium contained in a mixed precipitate of zinc and cadmium was the object in view.

		gram.			gram.
No. 1	gave CdO ...	0.0805	or	Cd ...	0.0704
„ 2	„ CdO ...	0.0615	„	Cd ...	0.0538
Differences		0.0190			0.0166

The next example given shows the result of experiments upon precipitates of the same nature, but containing a larger amount of each substance.

		gram.			gram.
No. 3	gave CdO ...	0.194	or	Cd ...	0.1697
„ 4	„ CdO ...	0.1165	„	Cd ...	0.1019
Differences		0.0775			0.0678

The differences here shown, taken in relation with the comparatively small amount of the metal present, are serious, and indicate the method as unreliable. The subsequent zinc-estimation in all of these cases showed that in *no instance* was the whole of the cadmium present precipitated; the error, therefore, relatively to the *total* cadmium present, was even larger than above shown. This was amply confirmed by other experiments, in all of which the solubility of the hydrated oxide of cadmium, and its subsequent precipitation with the zinc as sulphide, was inevitably found; in many instances the

quantity was so great as to impart quite a deep yellow colour to the otherwise white zinc sulphide.

Experiments were now made in which advantage was taken of the difference of behaviour of the two sulphides when boiled with a solution of potassium cyanide, sulphide of zinc being very *soluble* and the sulphide of cadmium *insoluble* in that reagent. This method appears, at first sight, to be both convenient and accurate. Several trials, however, showed that it was unsatisfactory; the difficulty of obtaining pure potassium cyanide free from iron, the subsequent destruction of the cyanide, and the large excess of potash salts left, with their effect upon the estimation of the zinc, gave results far from approaching the accuracy desirable.

After several trial experiments, the following method was adopted. It is based upon the difference of action of ammonium carbonate upon the precipitated metallic carbonates, and yields accurate and concordant results. From considerable experience it can be recommended by the author for the separation and individual estimation of the two metals.

The hydrochloric solution containing the two is evaporated to dryness on the water-bath, preferably in a platinum dish, so as to expel the excess of acid. The dried chlorides are then redissolved in about three times the amount of distilled water necessary for their complete solution; the aqueous solution is heated upon the water-bath almost to boiling, and a concentrated solution of pure sodium carbonate is added until there is a distinct alkaline reaction. At this temperature a granular precipitation of the carbonates of both metals occurs, which very quickly settles down and allows filtration almost immediately. After standing a short time the carbonates are thrown upon a filter and thoroughly washed, the nature of the precipitate rendering this very easy of execution. The washed carbonates are then re-transferred to the platinum dish with just a sufficient quantity of wash-water to completely remove them from the filter. If this be carefully done, it is by no means difficult to effectually remove the whole of the precipitate without loss from adherent portions. A saturated solution of pure ammonium sesquicarbonate is then added in considerable excess; after stirring, the mixture is allowed to stand in a warm chamber for about six hours. The whole of the zinc carbonate passes into solution; the cadmium carbonate, being insoluble, rapidly settles down. It is then thrown upon the same filter as before used, and washed: the filtrate and washings contain the zinc. After drying, it is ignited in a porcelain crucible and weighed as the oxide (CdO). Care must be taken to avoid the presence of any small particles of filter-paper

during ignition; otherwise reduction of the oxide will occur, and loss by volatilization as the well-known brown cadmium vapour. The filter-paper is saturated with ammonium nitrate to prevent volatilization of the small amount of adherent particles; it is then ignited in the usual way, either upon the lid of the crucible and not in contact with the oxide, or, more conveniently, in a smaller crucible: carefully conducted, little or no loss by reduction occurs. The filtrate containing the zinc is evaporated down to a small bulk to drive off the excess of ammonium salts, diluted to a convenient extent, and titrated with a standard solution of sodium sulphide in the usual manner.

The following examples may be given, to show the success of this method. 10 cub. centims. of solution of each of the pure sulphates containing known amounts were mixed, excess of ammonia added, and their complete precipitation effected by addition of ammonium sulphide in excess. The washed sulphides were then dissolved in hydrochloric acid, and the solution treated as above described. The following are the results obtained in each instance:—

No. 1.			gram.
Cadmium actually present.....			0·112
„ „ found			0·117
Error.....			0·005 +

	gram.
Zinc actually present	0·065
„ „ found	0·065

No. 2.			gram.
Cadmium actually present			0·1163
„ „ found			0·1239
Error			0·0076 +

	gram.
Zinc actually present	0·065
„ „ found.....	0·065

To ensure rigorous comparison in each of the above cases, the amount of each metal present in the 10 cub. centims. of solution taken was directly determined by taking at the same time a corresponding separate quantity of each of the solutions: the zinc solution was titrated with sodium sulphide, and the cadmium precipitated as the carbonate, ignited, and weighed as the oxide.

Other examples could be given, in which the error in the cadmium-estimation did not exceed 0·5 of a milligramme in 0·112 gramme total amount present; the zinc estimations were equally as favourable as the two examples above quoted. In

an extended series of separations of unknown quantities of the two sulphides the results were exceedingly concordant with each other, the relative quantity of each present being exceedingly variable.

Separation of Copper, Cadmium, and Zinc.—The methods of separation of copper from either cadmium or zinc, or both of these metals, were in most instances carefully examined. That which presents the most favourable features is by means of potassium sulphocyanide, being alike convenient and accurate.

The remarks upon the separation of zinc from cadmium by sulphuretted hydrogen in an acid solution apply in a similar manner to the present case, its separation from copper. Most processes which effect the separation of copper and zinc can be also applied in the case of copper and cadmium. Special attention was given to the two following methods of separation :—1st. That based upon the extremely sparing solubility of the sulphide of copper in a dilute solution of sulphuric acid (1 of H_2SO_4 + 5 of H_2O), and the complete solubility of the other two sulphides. 2nd. That founded upon the precipitation of the copper, in presence of the other two metals, as cuprous sulphide by the reagent sodium thiosulphate ($\text{Na}_2\text{S}_2\text{O}_3$). Neither of these was found convenient, or the separation by any means so complete as is desirable, especially when small quantities are required to be operated upon.

The sulphocyanide process was conveniently conducted as follows :—The nitric-acid solution containing the three metals, or any one of the remaining two with copper, was evaporated to dryness to expel the excess of acid. A sufficient quantity of sulphurous acid was then added to effect re-solution, with an equal bulk of distilled water. The potassium sulphocyanide was then added in the smallest possible excess; this is accompanied with the formation and precipitation of the pink-white cuprous sulphocyanide, $\text{Cu}_2(\text{CyS})_2$. After allowing to settle, the precipitate was thrown upon a filter and thoroughly washed; it was then retransferred to an evaporating-basin, dissolved in a small quantity of nitric acid, evaporated to dryness, redissolved in water, excess of ammonia added, and finally titrated with a standard solution of potassium cyanide in the usual manner. The filtrate and washings containing the cadmium or zinc, or both, was evaporated to a small bulk on the water-bath, and at this temperature precipitated by pure sodium carbonate. Their separation was then proceeded with as before described. If cadmium be present, its precipitate is washed, dried, ignited, and weighed as the oxide. If zinc only be present, it can be similarly treated, or redissolved in hydrochloric acid and titrated by sodium sulphide. Con-

siderable experience proved this method to be the most expeditious, convenient, and accurate of those usually employed for the separation of these metals.

The experiments were made in Prof. Galloway's laboratory at the Royal College of Science.

October 1879.

LIII. *Experimental Researches on the Temperature of the Sun.*
By F. ROSETTI, *Professor of Physics in the University of Padua.*

[Continued from p. 332.]

IV. *Calculation of the Constants of the Formula in order to render it applicable to the Experiments made with Pile No. 1.*

THE object of my investigation was to ascertain the temperature of the sun by applying the formula to experiments made on solar radiation. In these, and in many other experiments made on the radiation of very hot bodies, I employed the pile No. 1. The constants a and b , determined for pile No. 2, had then to be modified in such a manner that the same formula, with the new values of the two constants, could be applied to the experiments made with pile No. 1. For this purpose I made experiments by placing the two piles successively in front of the Leslie's cube, kept at a constant temperature of 312° C.

Pile No. 1 produced 28° deflection,
Pile No. 2 „ 78° „

giving

$$\frac{\text{No. 1}}{\text{No. 2}} = \frac{28}{78} = 0.359;$$

i. e. pile No. 1 = 0.359 No. 2.

Thus, in order to adapt the preceding formula to pile No. 1, the constants a and b had to be multiplied by 0.359 . I was desirous, however, of so expressing the formula, that by means of it the thermal effect of the sun on pile No. 1 could be directly compared with the effect of the radiation from the hot body observed under the same angle as the sun, *i. e.* $32' 3''.6$. By placing the pile with its face 134 millims. from the radiating surface of the Leslie's cube, which, by the interposition of a diaphragm, was limited to a circle of 32.65 millims. diameter, I obtained a deflection of 98.5 divisions on the galvanometer. In order that the radiating surface should be at the same angle as the sun, the pile should have been placed at a distance of 3.499 metres. By making use of the law of inverse squares, it is obvious that the deflection on the galva-

nometer should have been 0·1444 division. Thus we have for pile No. 1,

$$\text{No. 1} = \frac{0\cdot1444}{98\cdot5} = 0\cdot0014665.$$

The constants a and b have therefore to be multiplied by the two reduction coefficients 0·359 and 0·0014665 in order to obtain the new constants m and n . I have used the formula thus obtained in my experiments with pile No. 1. For this pile, then, we have the following formula:—

$$y = mT^2(T - \theta) - n(T - \theta),$$

$$\log m = 1\cdot2466088 - 10,$$

$$\log n = 5\cdot5254189 - 10,$$

on condition that the pile be placed at a distance from the radiating body equal to 107·17 times the diameter of the body, so that it shall appear, like the sun, at an angle of 31' 3''·6.

V. *Verification of the Applicability of the Formula for Temperatures above 300°.*

Calorimetric Experiments.—These experiments were made in order to ascertain whether the formula continues to represent correctly the thermal effect of the radiation of bodies when their temperature exceeds 300°. The first experiment was made with a sheet of copper covered with lampblack and heated by a spirit-lamp. This was one of the sources of heat employed by Melloni, according to whom the copper has a temperature of about 400°. Other physicists affirm the temperature to be 390°. On introducing the value of the deflection on the galvanometer, obtained with pile No. 1, into the formula, I obtained, from two successive experiments, numbers between 390° and 400°. As the temperature of the sheet was probably slightly different in the two experiments, I regarded the result of this first trial as satisfactory. It was necessary, however, that some more conclusive experiments should be made at temperatures which were higher, but which could nevertheless be determined by calorimetric methods. I therefore took a ball of copper heated to redness, the temperature of which was estimated by means of a calorimeter constructed for high temperatures. I suspended the copper ball by means of a fine platinum wire in the flame of a large Wiessnegg burner. The centre of the ball was in the line passing horizontally through the centres of the holes of the screw-nuts and through the centre of the face of the pile No. 1. In the circuit of the latter was included, as usual, the Wiedemann's galvanometer; and the temperature of the ball was considered to be constant when the galvanometric deflection remained

constant for a certain time. Then, at a given signal, an assistant pushed the flame aside, and another assistant placed the calorimeter-receiver under the ball and immediately raised it, so that the ball dipped into the water of the calorimeter. The work was all done so quickly that there was little fear about the cooling of the ball between the removal of the flame and the immersion in water.

The calorimeter employed was a double-walled vessel, provided with a thermometer (protected by a brass sheath) and a brass stirrer; the vessel also had a wooden handle, by means of which it could readily be moved. The thermometer was graduated into fifths of a degree, and had been compared with a standard. The water equivalent of the calorimeter with the thermometer and agitator had been determined by preliminary experiments. Half a litre of distilled water was poured into the vessel. If we call

Q the weight of the water poured into the vessel,

q the water equivalent of the vessel with thermometer and stirrer,

t_1 the temperature of the water before the immersion of the ball,

t_2 the temperature of the water after the immersion of the ball.

t the temperature of the heated ball,

p the weight of the ball,

c the mean specific heat of copper between t_1 and t_2 ,

we have

$$(Q + q)(t_2 - t_1) = pc(t - t_2),$$

from which the value of t can be calculated.

The quantity c has been determined by Bède (Wullner, *Physik*, vol. iii. p. 436, 1872), who found

$$c = 0.0910 + 0.000023(t + t_2).$$

The experiments from which Bède established this formula did not extend beyond 247° . For want of a better, I had to assume that the same formula might be employed even for much higher values of t . Of the numerous experiments which I made I shall only quote two.

In the first the surrounding temperature was 11° :—

$$\begin{aligned} Q &= 498.2, & q &= 32.5, & t &= 10.58, \\ t_2 &= 29.18, & p &= 123.33; \end{aligned}$$

by the two preceding formulæ we have

$$c = 0.1092, \quad t = 762^\circ.1.$$

In the second:—

$$t_1 = 11.15, \quad t_2 = 29.60, \quad t = 756^\circ.63.$$

Calculation of the Readings of the Galvanometer.—Four readings were taken on the galvanometer before the ball was immersed in the water. The first reading, with an open circuit, gave the zero-point ; the second was made with a closed circuit, the pencil of heat-rays being shut off by means of the double screen. These two readings were always nearly identical, showing that the case protected the pile effectually.

In the first of the experiments mentioned the readings were:—

Circuit open . . . 66·5

Circuit closed . . . 68·0

In the second:—

Circuit open . . . 64·0

Circuit closed . . . 65·5

The difference 1·5 in the two readings represents the effect of the medium upon the face of the pile which receives the radiation, the window of the case being open ; but it might also arise from some slight thermo-electric current induced by contact with the rheophore. Whatever may be the cause of the slight deflection, it can easily be eliminated by subtracting the second reading from the following readings which give the deflection produced by the radiation.

The third reading affords a measure of the thermal effect of the flame ; the difference between the fourth and the second reading gives the total effect of the radiation of the hot ball and of the half of the flame which surrounds it. Thus the thermal effect produced on the pile by the radiation of the ball can easily be found. It is well known that the blue flames of Bunsen's burners are almost entirely diathermanous. Consequently it is only necessary to subtract the number of divisions which represent the thermal effect of the front part of the flame (which on account of its feeble emissive power is always small in comparison with the number of divisions obtained by the radiation of the heated body) from the difference between the fourth and second reading, which gives the combined effect of the flame and the ball.

In the first of two calorimetric experiments the difference between the fourth and second readings was equal to 248·9 divisions, which, on subtracting 13·6 divisions, the thermal effect of that part of the flame which could radiate towards the pile, gives us 235·3 as the measure of the thermal effect of the ball on the face of pile No. 1.

The distance from the centre of the ball to the centre of the pile was 185 millims. ; and 66 millims. was the distance from the same centre to the screen, the smallest hole in which was 15 millims. in diameter. The diameter of the ball was 30 millims. It can be shown by a simple calculation that every

part of the pile received rays, not from the whole of the sphere, but from a spherical segment, the thermal effect of which was equal to the radiation which a circular disk of 21.42 millims. diameter would produce if placed so that the back point made a tangent to the sphere, *i. e.* at 170 millims. from the face of the pile. By the law of inverse squares, we find that if at 170 millims. distance the thermal effect of the radiation of the ball was 235.9 divisions of the galvanometer, then at 2296.5 millims. it should have been 1.322 division.

There are two methods of comparing the results of the calorimeter experiments with the galvanometer observations; and they yield similar results. We can either introduce the values T and $T - \theta$ (given by the calorimeter experiments) into the formula, and obtain thus the value of y , in which case we should have

$$T = 273 + 762.1 = 1035.1,$$

$$\theta = 273 + 11.0 = 284.0,$$

$$T - \theta = 751.1,$$

giving

$$y = 1.3943;$$

or we can equally well introduce into the formula the value $y = 1.322$ given by the galvanometer observations, and deduce from this the value of T and then of t . Doing this and remembering that the surrounding temperature was 11°C , and consequently

$$\theta = 273 + 11.0 = 284,$$

we obtain

$$T = 1019.0, \quad t = 746.0.$$

Comparing the two values of y and of t ,

$$\begin{array}{ccc} & \text{C.} & \text{G.} \\ y = & 1.3943 & \text{and} \quad 1.322, \\ t = & 762.1 & \text{and} \quad 746.0, \end{array}$$

we see that they do not differ much; but quite enough to show that the formula would not be applicable at high temperatures. It struck me that the slight difference might possibly be owing to the emissive power of the red-hot ball, and that, since the experiments which were employed for the calculation of the constants of the formula had been made with a surface covered with lampblack, all discrepancies would probably have disappeared, if even at high temperatures the radiating surface were covered with lampblack. I invented, therefore, a method of comparing the thermal effect of very hot bodies whose surface is bare with the same bodies when equally hot but with their surfaces covered with lampblack; so that, on comparing the two values, I obtained a number representing the emissive power of the plain body compared with the emissive

power of lampblack ; and taking the latter as unity, I thus obtained the emissive power of the body under experiment.

In the following section I shall make use of this method. For the present, I need only mention that I obtained from my experiments the number 0·943 to represent the emissive power of copper surrounded by a flame.

In the calorimetric experiments already mentioned, the bare ball of copper gave

$$y = 1.322.$$

If this ball had been covered with lampblack, the thermal effect would have been

$$y = \frac{1.322}{0.943} = 1.402 ;$$

and the formula would give the value

$$t = 763.6.$$

The values

$$\begin{array}{cc} 1.394 & \text{and} & 1.402, \\ 762.1 & \text{and} & 763.6 \end{array}$$

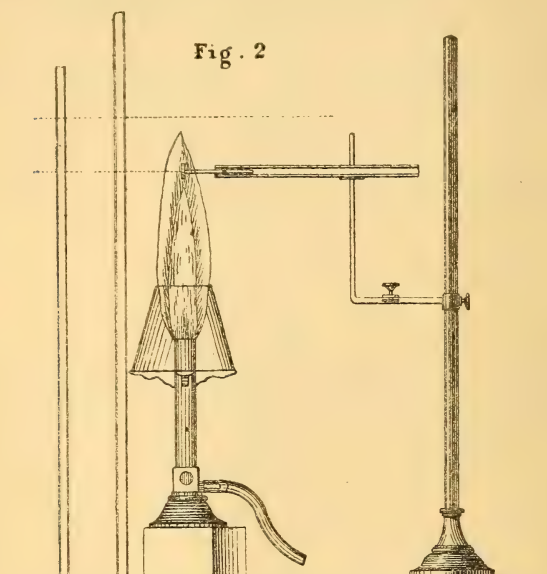
are near enough to be satisfactory ; and we may therefore conclude that the formula is applicable to high temperatures, provided that the radiating surface be lampblack, or that the specific emissive power of the radiating surface be introduced into it. I have made my description of this experiment very minute, in order to give an exact idea of the manner in which I have conducted these researches. I made many other calorimetric experiments, from which I obtained satisfactory results.

VI. *Method of determining the Specific Emissive Power of Bodies at High Temperatures.*

I have invented a very simple and exact method of determining the specific emissive power of bodies at high temperatures. Every body knows the form assumed by the flame of a Bunsen's burner when the air is freely admitted. By my researches on the different shapes of these flames ("Sulla Temperatura delle Fiamme," *Atti del R. Istituto Veneto*, vol. iii. 1877), I have shown that in the higher part lying between the apex of the interior cone and the summit of the exterior envelope the temperature is not higher than 1200°. A body placed in this space would have a temperature lower than that of the flame, dependent upon several causes—viz. the mass of the body, the specific heat, the conductivity, the emissive power, and the conductivity of the wire or rod employed to support the body. Nevertheless it is certain that, if the flame remains constant, the same body placed in the same position will have the same temperature in two successive experiments.

Now, if in one experiment the surface of the body be bare

and in the other covered with lampblack, it is evident that we thus have the means of obtaining the emissive power of the body under experiment compared with that of lampblack. The body must be placed exactly in the part of the flame mentioned; otherwise the lampblack will rapidly oxidize and expose the metallic surface (see fig. 2).



I made the bodies upon which I experimented into disks of 10 millims. diameter. They were kept in the centre of the flame by a wire or rod of the same substance held in a porcelain tube.

The results obtained for some of the bodies were as follows:—

<i>Copper.</i>				Div.	Ratio.
Dec. 6	{	Disk bare	105.9	}	0.950.
		„ blackened	113.5		
Dec. 6	{	„ bare	106.3	}	0.929.
		„ blackened	114.8		
Dec. 6	{	„ bare	114.4	}	0.939.
		„ blackened	121.8		
Sept. 12	{	„ bare	58.5	}	0.945.
		„ blackened	62.0		
Sept. 1					0.956.
Mean value for the emissive power of					0.943.
bare copper in the flame					

Iron.

		Div.	Ratio.
Sept. 11	{ Iron bare	77·75 }	0·888.
	{ „ blackened	87·5 }	
Sept. 12	{ „ bare	63·5 }	0·873.
	{ „ blackened	72·75 }	
Sept. 26	{ „ bare	116·0 }	0·886.
	{ „ blackened	131·0 }	
Mean value for emissive power of iron .			0·882.

Platinum.

		Div.	Ratio.
Sept. 11	{ Platinum bare	35·5 }	0·364.
	{ „ blackened	97·5 }	
Sept. 3	{ „ bare	18·75 }	0·338.
	{ „ blackened	55·5 }	
Sept. 26	{ „ bare	36·0 }	0·360.
	{ „ blackened	100·0 }	
Sept. 26	{ „ bare	81·0 }	0·337.
	{ „ blackened	240·0 }	
Mean value for emissive power of platinum .			0·350.

Cylinders of Magnesium Oxychloride.—As I proposed to make experiments upon the radiation of small cylinders of magnesium oxychloride when rendered incandescent by the oxyhydrogen flame, I wished to determine also the emissive power of this substance. The cylinders had a diameter of about 6 millims. I placed the one upon which I meant to experiment in a horizontal position so that its end penetrated into the Bunsen flame and occupied the same position as the disks in the preceding experiments; and I obtained the following results:—

		Div.	Ratio.
Sept. 12	{ Oxychloride mag. bare	15·5 }	0·596.
	{ „ „ blackened. . . .	26·0 }	
Sept. 3	{ „ „ bare	14·8 }	0·580.
	{ „ „ blackened. . . .	25·5 }	
Sept. 26	{ „ „ bare	23·9 }	0·572.
	{ „ „ blackened. . . .	41·6 }	
Mean value for emissive power of magnesium oxychloride			0·584.

Therefore, at high temperatures, the emissive powers of Cu, Fe, Pt, and Mg. oxychloride are represented by the numbers 0·943, 0·882, 0·350, 0·584.

VII. *Verification of the Applicability of the Formula to Temperatures above 800°.*

If we call ϵ the specific emissive power of the radiating bodies, the formula becomes

$$y = m\epsilon T^2(T - \theta) - n(T - \theta);$$

and if the experiments be made with lampblack, $\epsilon = 1$.

In order to ascertain whether this formula be also applicable when the temperatures are higher than those used in the calorimetric experiments, I used the same experiments as those by which ϵ was determined, commencing with copper. I made use successively of three disks of copper with the same diameter of 10 millims. The first had a thickness of 1 millim., the second of 2 millims., and the third of 3 millims. It is certain that each different disk when placed in the flame acquires a different temperature, the smallest attaining a higher temperature than the other two: if a disk were made any smaller (as, for example $\frac{1}{4}$ of a millimetre), it would soon become red hot and begin to melt. Now we know the melting-point of copper is 1050° ; and therefore we may conclude that the smallest of the three disks acquires in the flame a temperature of about 1000° . The following results were obtained:—

Sept. 3.—Copper disk blackened, with thickness 1 millim., diameter 10 millims., placed at 438 millims. from the pile; the surrounding temperature being 28°C. ; $\theta = 273 + 28 = 301^\circ$; the deflection on the galvanometer = 15.9. Calculating the deflection on the galvanometer, supposing the pile were at a distance of $107.17 + 10$ millims., we obtain

$$y = 2.6554 \text{ divisions.}$$

Introducing this value into the formula, as in the case of $E = 1$, we have

$$T = 1260^\circ, \quad t = 987^\circ.$$

In another experiment I obtained:—

Sept. 26.—Copper blackened; thin disk (1 millim.); distance from the pile 163 millims.; surrounding temperature 18° ; $\theta = 291^\circ$; deflection of the galvanometer = 108.0:

$$y = 2.498, \quad T = 1233^\circ.5, \quad t = 960^\circ.5.$$

I made two experiments with the disk of thickness 2 millims.:—

Dec. 1.—Copper blackened; thickness of disk 2 millims.; distance from the pile 150 millims.; surrounding temperature 11° ; $\theta = 284^\circ$; deflection of galvanometer 121.8:

$$y = 2.386;$$

the formula gives

$$T=1211^{\circ}, \quad t=938^{\circ}0.$$

Dec. 6.—Distance from the pile 157 millims.; $\theta=284^{\circ}$; deflection = 113·5.

$$y=2\cdot432, \quad T=1218^{\circ}, \quad t=945.$$

Disk of copper 3 millims. thickness:—

Dec. 1.—Distance from the pile 150 millims.; $\theta=284^{\circ}$; deflection = 114·3:

$$y=2\cdot239, \quad T=1190^{\circ}7, \quad t=917^{\circ}7.$$

Dec. 6.—Distance from the pile 157 millims.

$$y=2\cdot272, \quad T=1196^{\circ}3, \quad t=923^{\circ}3.$$

The temperatures 987° and 960° obtained with the thin disk, 938° and 945° , with the medium disk, and $917^{\circ}7$ and $923^{\circ}3$ with the thickest disk, were most probably their true temperatures at the time of the experiment.

Iron.—The disk of iron was very thin, 10 millims. in diameter, and was covered with lampblack.

Sept. 26.—Distance from the pile 163 millims.; deflection $131\cdot0$; $\theta=291^{\circ}$:

$$y=3\cdot030, \quad T=1308^{\circ}0, \quad t=1035^{\circ}.$$

Platinum.—The disk of platinum was very thin; diameter 10 millims.:

$$y=3\cdot847, \quad T=1410^{\circ}, \quad t=1147^{\circ}.$$

Cylinder of Oxychloride of Magnesium.—Diameter 6·1 millims.; distance 163 millims.; deflection $41\cdot6$; $\theta=291^{\circ}$:

$$y=4\cdot091, \quad T=1432^{\circ}5, \quad t=1159^{\circ}5.$$

To sum up, we find that the disks of copper acquired different temperatures according to their thickness: the thinnest disk attained the temperature of 987° , the next thicker one 945° , and the thickest 923° . The thin disk of iron reached the temperature of 1035° , the disk of platinum 1147° , and the cylinder of magnesium oxychloride 1160° . We can understand how the last-mentioned body may attain a higher temperature than any of the others; for, being non-metallic, it is not so good a conductor of heat as the others, and therefore does not give up as much of its heat to the metallic support or porcelain tube by means of which it is held.

Experiments made with a Cylinder of Magnesium Oxychloride rendered Incandescent by means of a Flame of Coal-gas and Oxygen.

The cylinder of magnesium oxychloride which I used in my experiments had a diameter of 6·1 millims. The flame em-

ployed to heat the cylinder was very short ; but the heat from it was so intense that the platinum flowed like wax. It must be understood that in all these experiments the second reading on the galvanometer was subtracted from the first—the latter representing the effect of the radiation of the cylinder and flame, the former the radiation of the flame and surrounding bodies. It should also be remembered that a double screen with a hole in it a little larger than the base of a cylinder was placed between the latter and the pile, so that the influence of surrounding bodies was very small. The following are the results which were obtained.

First Series.

Cylinder of Magnesium oxychloride bare ; diameter 6·1 millims. ; surrounding temperature $24^{\circ}4$; distance from the pile 588 millims.

First experiment.—Deflection of the galvanometer 11·2 divisions.

We find by calculation that, if the distance of the pile had been $6\cdot1 + 107\cdot17$ millims. (in order to make the radiating surface appear as if at an angle of $32' 3''\cdot6$), the reading would have been $y_1 = 9\cdot051$ divisions. But if we remember that the emissive power of magnesium oxychloride is 0·584, we find that, if the cylinder had been covered with lampblack (*i. e.* if its emissive power had been equal to 1), the thermal effect would have been equal to

$$y = \frac{y_1}{E} = \frac{9\cdot051}{0\cdot584} = 15\cdot499 \text{ divisions.}$$

Introducing this value of y into the formula, if $t_1 = 24^{\circ}4$ we shall have $\theta = 297^{\circ}4$, and we thus obtain $T = 2169^{\circ}$ and $t = 1896^{\circ}$.

Second experiment.—Under the same conditions as the first. Deflection 12·3 millims., giving $y_1 = 9\cdot9404$ millims.,

$$y = \frac{y_1}{E} = \frac{9\cdot9404}{0\cdot584} = 17\cdot06.$$

By the formula we obtain

$$T = 2236^{\circ}, \quad t = 1963^{\circ}.$$

Third experiment.—Reading on galvanometer 12·2 divisions.

$$\begin{aligned} y_1 &= 9\cdot859, & y &= 16\cdot84, \\ T &= 2227^{\circ}\cdot5, & t &= 1954^{\circ}\cdot5. \end{aligned}$$

Second Series.

First experiment.—The light was dazzling. Distance from

the pile 420 millims.; surrounding temperature $24^{\circ}0$; reading on the galvanometer 31.6 divisions.

Calculated for the distance $6.1 + 107.17$,

$$y_1 = 13.0 \text{ divisions, } y = \frac{y_1}{0.584} = 22.25;$$

the formula gives

$$T = 2440^{\circ}, \quad t = 2167^{\circ}.$$

Second experiment.—In this experiment I obtained a large deflection on the galvanometer, but the cylinder shone very brilliantly. Distance from the pile 420 millims. Deflection (mean of three experiments) = 42.0 divisions. At the distance $107.17 + 6.1$ millims. we should have had

$$y_1 = 17.68 \text{ divisions, } y = \frac{17.68}{0.584} = 29.58,$$

$$T = 2670^{\circ}, \quad t = 2397^{\circ}.$$

These two numbers, 2167° and 2397° , although higher than those obtained in the first series, will not appear too high if we reflect that platinum immediately melted before the flame, and that the light emitted in the experiments of the second series was considerably greater than that emitted in those of the first. I think that all who have made researches of this nature will consider the results obtained to be satisfactory, and that there is reason to conclude that the formula

$$y = mET^2(J - \theta) - m(J - \theta),$$

which has stood the tests so well up to 2000° , can confidently be applied to experiments made at higher temperatures, and that it can be employed to find the temperature of the sun.

[To be continued.]

LIV. *On Magneto-Electric Induction.* By FREDERICK GUTHRIE and C. V. BOYS, *Assoc. R. School of Mines**.—Part I.

[Plate XIII.]

IT is well known that the electric currents caused in a conductor by relative motion between that conductor and a magnetic pole are in such a direction as to impose a drag upon such motion. An ideal friction (that is, one without recoil) is called forth; and, accordingly, in the experiments sometimes called Arago's such current-friction exhibits itself in the pursuit of the moving element by the one which is free to move, though originally at rest. Again, the same current-

* Communicated by the Physical Society.

friction appears in the damping of the swing of the compass-needle by neighbouring metallic masses, and by the evolution of heat in the metallic disk revolving between magnetic poles.

It occurred to one of us that such pursuit as occurs in Arago's experiment might be made use of with advantage to measure the rate at which machinery is moving. For we have merely either (1) to connect a revolvable magnet with the machinery, and measure the angle through which it turns a copper plate in its near neighbourhood, which is restrained by increasing torsion, spring, or weight moment, or (2) to connect a copper plate with the machinery so as to revolve in the neighbourhood of a magnet restrained by similar means or by the earth's directive magnetism. The latter plan indeed might seem to have the obvious advantage that, as in the tangent-galvanometer, the amount of the effect is independent of the strength of the magnet. But this advantage is probably more than counterbalanced by the limitation of the angle of deflexion to less than a right angle, whether the magnet swing in a horizontal or a vertical plane.

We have in the following experiments adopted the plan of making a permanent steel or electromagnet revolve in the neighbourhood of the conductor, and governing the motion of the latter by the torsion either of a hair spring of a watch or fine platinum wire.

The accuracy of measurement which can thus be reached, and the greatness of the effects, led us to the hope that the electrical conductivity of liquids might be detected and measured by the same means—a result much to be desired, because, while the effect is due to the passage of currents in the liquid, such passage is wholly unaccompanied by electrolysis and its attendant incubus, polarization.

Before undertaking this latter investigation we have re-examined experimentally the results previously obtained with metallic conductors by others; and as our method differs somewhat from those previously employed, and our results are more extended and in some cases at variance with those of former experimenters, we venture to lay them before the Society*.

Our own Experiments.

Our experiments were mapped out as follows:—

Other things being the same,—

I. Vary rate of rotation.

II. Vary distance between the elements.

* At the recommendation of the Publication Committee of the Physical Society, we omit an historical summary of the previous investigations of others.

III. Vary diameter of disk.

IV. Cut metallic disk.

V. Vary thickness.

VI. Vary nature of metal of disk.

VII. Examine liquids in view of the determination of their conductivities.

I. *The Relation between Velocity and Deflexion.*—The apparatus used for establishing this relationship is, in its later form, shown in Pl. XIII. fig. 1 one third the true size, the difference being that the torsion-thread was held by a clip attached directly to a glass shade instead of to the sliding arrangement shown.

The motive power was a Froment's electro-magnetic engine. The elastic band from the engine passed round one or other of the wheels on the vertical spindle, p , carrying a pair of magnets, ns , each $3\frac{3}{8}$ inches long $\times 1\frac{1}{8} \times \frac{1}{8}$, placed with their similar poles together. A screw on the spindle geared with a worm-wheel, w , with ninety-seven teeth. The time of a revolution of this could be measured either by watching a mark on the wheel pass a fixed pointer, or by its making a contact every revolution so as to sound a bell. The torsion-thread used was the hair spring of a watch, to which was attached the copper disk by means of a screw clip. The speed was altered by placing the driving-band on one or other of the pulleys on the vertical shaft, or on the Froment engine, or by altering the strength of the battery driving it, or by means of a friction-break.

The disk was graduated into degrees on its upper face, and the reading made by keeping a vertical edge, the eye, and the torsion-thread in one plane, and noting the division or part of one intersected by that plane, so as to avoid parallax.

When the deflexion of the disk exceeds 20° or 30° the wire takes a "set," especially if it is kept long at its maximum twist. It appears completely to recover itself in time; but such recovery is at last so slow that it is preferable to re-determine the zero immediately after each experiment, and to allow for this, assuming that at the moment of observation (that is, when the disk is at its greatest excursion) the set in the wire is the same as it is immediately after when the zero is re-determined. When the deflexion is considerable it is impossible to keep it absolutely constant; accordingly the mean was taken of several maxima and minima, and the rate was registered after each set of observations. The means of the means of deflexion and rate were finally taken. When the rate is high and the deflexion considerable, the disk begins to swing, the axis describing a cone, until reading becomes impossible.

This is possibly due to the vertical repulsion between the moving magnet and the disk not being quite symmetrical. In Experiment 1 all the data are given; in Table I. the means only are given.

Experiment 1.

O = 0.

Maximum deflexion.

Minimum deflexion.

368 ⁰	363 ⁰
365	362
366	359
367	357
367	359

Index-wheel went five times round in 49 seconds.

364	359
363	360
364	361

Index-wheel went five times round in $49\frac{1}{4}$ seconds.

366	357
365	355

O = + 2°.

The index-wheel going round once corresponds to ninety-seven revolutions of the magnet.

Allowing 2° for the effective set, we have a deflexion of $360^{\circ}\cdot 25$ caused by a velocity of 9·825 revolutions a second. In Table I. in the first five lines the rate-observations were simultaneous with the angle-observations; in the latter six they were taken as in the example given: each result is the mean of about eight observations.

TABLE I.

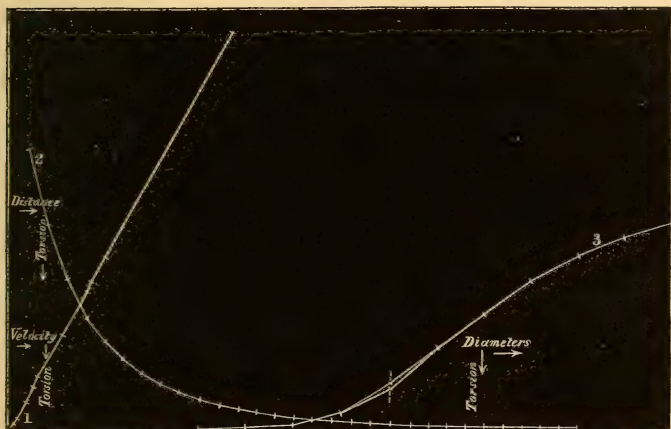
Angular-velocity, rotations
per second.

Angular torsion.

0·258	9·7
0·527	19·3
1·672	60·5
1·702	61·3
2·517	91·2
2·900	106·9
5·374	193·9
7·886	285·9
8·186	303·9
9·877	360·3
23·307	840·0

It appears from these numbers and from their graphic representation in line 1, fig. A, that the torsion varies directly

Fig. A.



as the rate. So exactly is this the case that we may in future with perfect confidence reduce from one rate to another or to any arbitrary common rate when the rate varies between experiments, to test the effect of variation of other kinds. And we have, if the magnets and torsion keep constant, a very exact and not inconvenient instrument for measuring the rate of rotation of machinery.

II. *The Relation between Distance and Deflexion.*—Two series of experiments were made to determine this relationship. We need only reproduce here the longest and most complete. The apparatus used was that shown in fig. 1, Plate XIII., which is drawn to a scale one third true size.

The copper disk, *c*, was replaced by one of ebonite, to the lower surface of which the metal plate could be attached by a drop of weak gum. The copper disk used was only 0.27 millim. thick ; so that all its parts might be considered at the same distance from the magnet. The magnet used was $3\frac{5}{16} \times \frac{1}{2} \times \frac{3}{16}$ inch. The upper end of the torsion-thread was attached by a screw-clip to the brass rod, and graduated in millimetres. This could be slid up and down, so as to vary the distance between the magnet and the disk. To get the absolute distance, the mica screen between them was removed, and a piece of copper exactly 1 millim. thick laid on the magnet, and the disk lowered till it just began to rest on the copper. Then the copper was replaced by the mica, and the

first experiment made. Then the disk was raised 1 millim. by means of the graduated rod *r* for each succeeding experiment.

The ebonite disk and central clamp had been found to be unaffected by the fastest speed used. In order to avoid extravagant torsion, the rate of rotation was made less when the distance was small.

In Table II., column D shows the distance in millimetres between the upper surface of the magnet and lower surface of the copper disk; A shows the observed angles of depression; T shows the time in seconds for one revolution of the worm-wheel; A' is the product of A and T. Since T is inversely and A is directly as the rate, A' shows the pure effect of distance; it would be a constant if the distance did not vary. Each of the experimental numbers given, whether of angle or time, is the mean of never less than three observations. These observations of angle and time were simultaneous.

TABLE II.

D.	A.	T.	A' = A × T.
1	106.8	13.7	1463.2
2	78.0	13.7	1068.8
3	75.6	10.2	771.1
4	57.0	10.1	575.7
5	68.3	6.5	443.9
6	50.9	7.2	366.5
7	55.1	5.3	292.0
8	43.3	5.3	229.5
9	36.3	5.3	192.4
10	{ 45.1 44.6	{ 3.6 3.6	{ 162.4 160.6
11	36.1	3.7	133.6
12	30.5	3.7	112.8
13	25.6	3.5	96.6
14	21.0	3.6	75.6
15	17.9	3.4	60.9
16	15.5	3.5	54.25
17	13.3	3.6	47.88
18	11.6	3.5	40.60
19	10.4	3.5	36.40
20	{ 9.0 10.0	{ 3.5 3.6	{ 31.50 36.00
21	8.0	3.5	28.00
22	7.1	3.5	24.85
23	6.2	3.5	21.70
24	5.2	3.5	18.20
25	4.8	3.4	16.32
26	4.6	3.4	15.64
27	3.5	3.6	12.60
28	3.5	3.6	12.60
29	3.0	3.6	10.80
30	2.6	3.6	9.36

The second numbers opposite distances 10 and 20 millims.

were obtained after the completion of the set of experiments, by resetting the plate to the proper distances, to see if there had been any great change in the power of the magnets or the torsion of the thread.

On plotting out the curve of distance we find line 2, fig. A (p. 453).

III. *Variation in the Diameter of the Disk.*—For the study of this effect the same apparatus was used again, the only difference being that the magnet was only 2 inches long ; and a series of disks of copper were cut of the same thickness as that used for finding the law of distance. They were held in the same way—by a drop of weak gum on the under surface of the ebonite disk. The distance was kept constant, namely 3 millims. In Table III., column Diam. gives the diameter of the disk in inches, column A the corrected mean deflexion of about four observations, column T the time of one revolution of the worm-wheel, got by taking the time of ten or twenty turns, during which the angles are read ; and column A' is equal to $A \times T$, and represents the torsion for constant velocity.

TABLE III.

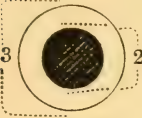
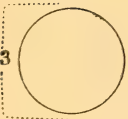
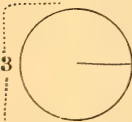
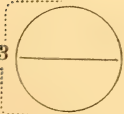
Diam.	A.	T.	A' = A $\times$ T.
1.00	0.3	2.53	.759
1.25	0.9	2.79	2.511
1.50	2.6	2.82	7.332
1.75	6.5	2.83	18.395
2.00	{ 10.7	{ 3.99	{ 42.69
	{ 12.5	{ 3.90	{ 48.75
2.25	{ 21.5	{ 4.02	{ 86.43
	{ 21.4	{ 4.09	{ 87.53
2.50	30.8	4.03	124.1
2.75	38.7	4.06	157.1
3.00	46.1	4.01	184.9
2.25	50.9	4.00	203.6
3.50	50.0	3.90	218.4

The curve 3 in fig. A is the graphic representation of the above relationship. As was to be expected, there is a point of contrary flexure at about the region where the disk has a diameter equal to the length of the magnet.

IV. *Effects of Cuts in Disks.*—The same apparatus was used, and copper disks 3 inches in diameter, of the same thickness and at the same distance from the 2-inch magnet. In Table IV., A is the observed angle, T the observed time of rotation, and A' is the product of A and T, and is proportional to the force for a constant rate.

The attached figures represent the way in which the disks were cut.

TABLE IV.

		A.	T.	$A' = A \times T.$
1.		44.0	4.03	177.3
2.		2.8	4.07	11.4
3.		9.6	4.37	41.95
4.		16.0	4.31	68.96
5.		1.8	4.44	8.0
6.		39.9	4.42	176.4
7.		37.6	4.35	163.6
8.		33.0	4.32	142.6
9.		30.6	4.24	129.7
10.		26.7	4.24	113.2

53.4

77.0

In this Table the dark spaces in 2 and 4 mean absence of metal. The sum of the torsional effects of 2 and 3 or of 4 and 5 is equivalent to that of the disk in 1 having a concentric circular cut 2 inches or $1\frac{1}{2}$ inch in diameter respectively—from which it will be seen that a concentric circular cut interferes with the induced currents far more than radial cuts of equal or even greater length. Experiment 6 is a repetition of experiment 1. The effect of the radial cuts in 7, 8, 9, and 10, though agreeing in kind with the experiments of Babbage and Herschel, is far less in amount; and this is noteworthy, as in their experiments the disks of lead were not cut completely to the centre, but a small space was left to unite the different parts together; while in our experiments the copper disk was cut completely through, the different parts being held in position by the ebonite plate above.

Taking the torsional effect of an uncut disk as 100, the following numbers show the effect of disks cut as in 7, 8, and 10, as obtained by

	7.	8.	10.
Babbage and Herschel	83.2	72.5	44.8
Us	92.8	80.7	64.2

V. *Effect of Thickness.*—The examination of the effect of, thickness is experimentally involved in that of distance, because, in increasing the thickness of a plate, it is impossible with our data to avoid change of distance of plane of effective mass. We have thought it best to employ for this purpose very thin disks cut out of a uniform sheet, and stuck with thin gum under the ebonite disk, at first one, then two, and so on up to six. Tinfoil was used 0.13 millim. thick, and 3.625 in. in diameter.

TABLE V.

No. of foils.	A.	T.	$A' = A \times T.$	$\Delta A'.$
1.	4.9	22.7	111.2	111.2
2.	10.5	21.3	223.6	112.4
3.	15.9	21.3	338.7	115.1
4.	22.0	21.3	468.6	129.9
5.	26.6	22.3	593.2	124.6
6.	30.0	24.6	738.0	144.8
1a.	6.3	23.2	146.2	

The column $\Delta A'$ shows the increase in effect due to each foil; it also shows that the lower foils, being nearer the magnet, are more effective than the upper ones. A single foil 1a was afterwards hung at the distance of foil no. 6, and gave the result 146.2, agreeing with 144.8. The effect, then, of disks

may be taken as directly proportional to their joint thickness the mean distance remaining the same. There is probably a misreading with the four disks.

VI. *Effect of Nature of Metal.*—Sheets of copper, zinc, tin, brass, and lead having been rolled between the same rollers, were found to have the thickness as under :—

	millim.
Copper	0·89
Zinc	0·87
Tin	0·71
Brass	0·93
Lead	0·76

Disks 2·875 inches in diameter were cut out of the sheets, and fastened in the usual way by a drop of gum to the under surface of the ebonite disk. The results are given in Table VI.

TABLE VI.

Metal.	A.	T.	$A'=A \cdot T$.	$\frac{A'}{t}$.	r .	$\frac{A' \times r}{t}$.
Copper...	78·5	21·8	1711·3	1923	·119	229
Brass ...	28·8	23·0	662·4	712	·423	301
Zinc ...	31·4	23·0	722·2	830	·326	271
Tin ...	13·0	23·3	302·9	427	·669	286
Lead ...	8·4	23·0	193·2	254	1·109	281

The column headed $\frac{A'}{t}$ is obtained by dividing the numbers in the previous column by the thickness of metal used. The numbers in the column r , give the resistance in ohms of 100 inches of wire drawn from the same metal that was rolled into sheets, and ·79 millim. in diam. These two columns multiplied together give the last column, which should be a constant if the deflexion is proportional to the conductivity. Now these numbers range from 229 to 301, which seems rather a wide divergence ; but in this case there are many considerations to be taken into account, in each of which errors may easily creep in, especially in the assumption that the specific conductivity of the same metal is the same whether it be rolled or drawn. The approximation to equality, obtained in the numbers in the last column, is all that could be expected.

It appears from the above experiments in I., II., III., IV., V., VI., that the torsion varies directly with the moment of the current.

VII. *Examination of Liquids.*—The electro-magnet seen in vertical section in Pl. XIII. fig. 2, one third true size, was made to screw onto a vertical axis, so as to revolve in a horizontal plane.

A 5-in. sulphuric-acid dish was held just above it by a separate stand to avoid any effects that vibration might cause ; and into this dish the liquid to be tested could be put ; a cover was also put over the dish. Mercury was first tried ; and it soon began to rotate in the same direction as the magnet. Dilute sulphuric acid of the best-conductivity strength was next experimented on : a wood float that had been soaked in paraffin to make it float in the middle of the vessel served for an index to show motion of liquid. The float followed the magnet at the rate of about 15° an hour, but was always rather uncertain in its movement. The only noticeable feature of this experiment was, the fine sulphate-of-lead powder which had settled during the night arranged itself during the experiment in fine concentric rings, the circle immediately above the poles of the electro-magnet being bare. A solution of sulphate of copper showed no indications whatever.

As this method did not seem at all promising, the apparatus shown in vertical section in fig. 2 was next used. A glass bulb B was filled with the same sulphuric acid that was used in the last experiment, and hung by a silk thread in a small beaker *c* which acted as a screen ; there was also a screen above. The electro-magnet *e* was made to revolve before the current was turned on, to see if there was any mechanical effect ; but there was none at all ; as soon, however, as the wires were connected with the battery the bulb B began to swing round, and stopped at about 180° . Here there was an unmistakable effect obtained with a non-metallic liquid conductor.

If, then, the torsion is proportional to the conductivity of the liquid in the vessel, which it undoubtedly is, we have the means of measuring the conductivity of liquids without using electrodes, and therefore without polarization. To see how the method worked, the preliminary apparatus shown in figs. 3 and 4 was constructed. NS is a double electro-magnet made from iron bar 1 in. wide and $\frac{1}{4}$ thick, bent as shown in horizontal section in fig. 3, so that the two poles are inside : with this arrangement the lines of force which intersect the cylindrical vessel B are almost straight and parallel. The vessel B was hung in the cylindrical screen *c* by a platinum wire 15 ft. long and 0.002 in. in diameter ; and a small mirror was fixed to it for observing the reflexion of a small gas-flame. The liquid could be introduced by a pipette through a small hole in the side. Mixtures of sulphuric acid and water were made in the following proportions by weight, and were tested in the above apparatus:—

	<i>a.</i>	<i>b.</i>	<i>c.</i>	<i>d.</i>	<i>e.</i>
Acid	7	6	5	4	3
Water	3	4	5	6	7

They gave the following numbers, which represent the number of divisions on a scale of equal parts, at which the reflexion of the light could be seen when the mirror was in its position of mean deflexion.

a 2.1, b 2.6, c 5.5, d 6.8, e 8.9.

These numbers, being obtained only from a provisional apparatus, must not be considered to represent the accuracy of the process, but merely as showing that larger and more perfect apparatus is likely to give good results.

With regard to the validity of this method of determining the conducting-power of liquids a few words may be said. 1st. What will be the action of para- or dia-magnetic liquids apart from conductivity? Iron gives a greater turning effect than copper when placed over a revolving magnet, owing to its very great magnetizability and to its retaining magnetism a perceptible time; but whether liquids can acquire sufficient magnetism and retain it long enough to vitiate the results it is impossible to say; experiment will show. 2nd. Does the torsion of the wire really represent the twisting effect on the liquid *at rest*? It would seem at first that it does not; but a little consideration will show that no error in any way appreciable can be committed on the supposition that it does. The action is this: the rotating magnets cause the liquid in the vessel to rotate in the same direction; the friction between the liquid and the vessel tends to make the vessel rotate too; but it is restrained by the torsion of the wire. As the inertia of the liquid has to be overcome by a comparatively small force, its velocity will increase very slowly; and were there no friction against the sides of the vessel it would, if only left long enough, equal that of the magnet. But there is friction, which increases with the velocity; and therefore as the velocity increases the acceleration will diminish, and in time the velocity will be constant—that is, when the twisting force and the friction become equal—just as a light body when dropped in air soon attains a constant velocity. This velocity depends on the viscosity of the liquid; and it might be well said that two liquids having the same conductivity, but one viscous and the other mobile, would attain very different constant velocities, and, as the twisting force is directly proportional to the *relative* motion between the liquid and the magnet, that different results would be attained. This is quite true; but the question turns upon the velocity which the liquid attains. If it is itself comparable to the velocity of the magnet, then any changes in it will affect the result but if it is nothing compared with the magnet's velocity, then no error at all can be produced. Now on looking back it will be seen that the rotation of the

best-conductivity acid in the 5-in. basin over a revolving electro-magnet turning about 3000 times a minute, was about 15° an hour; in other words, the magnet went about 4,320,000 times as fast as the liquid; and the total possible error on the supposition that the liquid is still, would be $\frac{1}{4320000}$ of the whole amount; therefore it may be safely neglected.

It appears, therefore, that the point to be borne in mind is that the torsion must not be measured till the liquid has attained its constant velocity; *i. e.* sufficient time must be allowed. This velocity and this time will be less the greater the friction or viscosity. But the introduction of glass threads, screens, or porous plates to increase the friction would do more harm by introducing uncertain electrical resistances than they could possibly do good: the only device which seems as if it might be of any use would be to make a jelly of the conducting liquid; but this is quite unnecessary.

We are engaged in measuring the conductivity of liquids by means similar to those above shown.

EXPLANATION OF FIG. A (page 453).

Curve 1. The abscissæ are in proportion to the velocities of rotation of a permanent steel magnet beneath a copper disk; the ordinates are in proportion to the angles at which the disk comes to rest. This curve appears to be a straight line, showing that the torsional moment is proportional to the velocity.

Curve 2. The abscissæ are in proportion to the distances between a thin copper disk and the upper surface of a permanent steel magnet revolving beneath; corrected for rate according to curve 1, the ordinates are in proportion to the angles at which the disk comes to rest.

Curve 3. The abscissæ are in proportion to the diameter of a thin copper disk, varying from 1 in. to 3.5 in. in diameter, turned by a revolving permanent magnet beneath, 2 in. long; the ordinates are in proportion to the angular deflexions. The double line shows the secondary observations about the region (the "pole") of the magnet where the effect of variation of diameter should be most marked. About this place there is an alteration in flexure.

LV. *Influence of Atomic Weight.* By THOMAS CARNELLEY, D.Sc., Assistant Lecturer on Chemistry in the Owens College.

[Concluded from p. 381.]

VISCOSITY, or the *Transpiration of Liquids*.—This is the term applied to the passage of liquids through a fine capillary tube under pressure. Poiseuille, who first made experiments on this subject, was unable to find any connexion between the rate of flow and the chemical composition of the substance, because he used percentage solutions instead of solutions pro-

portional to the equivalent of the body dissolved. Graham (Phil. Trans. 1861, p. 373; Chem. Soc. Journ. xv. p. 427) afterwards found, as regards alcohols, ethers, and organic acids belonging to the same homologous series, that the rate of flow was the greater the lower the body was in the series. He also found that the rate of an acid is slower than that of an ether with which it is metameric. More recently still, Schultze (*Chem. Centr.* 1872, p. 705) found, as the result, of his experiments, that in respect to solutions of salts of K, Na, and Li, as also to solutions of salts of Ba, Sr, Ca, and Mg, the velocity of flow is the greater the greater the atomic weight of the metal, and that there is but little difference between the rate of flow of the chloride, bromide, and iodide of one and the same metal. But it was reserved for Hannay (*Nature*, 1879, xix. p. 378) to show that solutions containing equivalent quantities of the respective salts must be used if the influence of the chemical composition on the rate of flow is to be fully made out. "By the use of such normal solutions, he found that the rate of flow does not depend on any of the mechanical properties of the salts, such as crystalline form, molecular volume, solubility, &c., but upon the mass of the elements forming the substance and the amount of energy expended in its formation. Each element has a value of its own, which is continued in all its compounds. Thus all the salts of K and Na formed by the same acids have a constant difference. In like manner, each metalloid and acid radical has a value which is continued in all its combinations. The greater the combining value of the element, the quicker its rate of flow. Thus K has a higher rate than Na, Ba than Sr, and so on. The rate of flow also varies with the amount of energy in the compound: thus nitrates stand highest, as they contain most energy, then chlorides, and lastly sulphates, which are exhausted compounds." It is therefore evident from these facts that the chemical equivalents may be found by determining the rate of flow of their compounds.

In a paper read before the Vienna Academy (*Nature*, 1879, xx. p. 277) on the specific viscosity of liquids and its relations to chemical composition, Pribram and Handl have shown:—(1) That the substitution of Cl, Br, I, or NO₂ for H in a molecule causes, in all cases, an increase in the time of flow through a capillary tube. (2) That this increase is least on the substitution of Cl, and more successively in those of Br, I, and NO₂. (3) That the increase in the rate of flow depends not only on the nature of the element, but also on its position in the molecule.

Capillarity.—I have already referred to the fact (*vide supra*,

p. 308) that those metals with the highest melting-point have also the greatest constant of capillarity in the *solid* state; but if any connexion exists between the capillarity and chemical composition of *liquid* inorganic bodies, it is an extremely complicated one, and very difficult to trace. Gueront (*Compt. Rend.* lxxxiii. p. 1291), however, has recently shown, as regards homologous series of liquid organic compounds, that the coefficient of capillarity decreases as the quantity of C increases. This he has proved in the case of the fatty acids, the ethyl ethers of the fatty acids, and the acetic ethers, thus:—

		Coefficient of capillarity.	Boiling- point.
Formic acid,	$\text{CH}_2 \text{O}_2$	115·0	101
Acetic „	$\text{C}_2 \text{H}_4 \text{O}_2$	160·5	118
Propionic acid,	$\text{C}_3 \text{H}_6 \text{O}_2$	189·0	141
Butyric „	$\text{C}_4 \text{H}_8 \text{O}_2$	129·5	162
Valerianic „	$\text{C}_5 \text{H}_{10} \text{O}_2$	92·3	175
Caproic „	$\text{C}_6 \text{H}_{12} \text{O}_2$	64·0	205
Ethyl formate,	$\text{C}_3 \text{H}_6 \text{O}_2$	542·0	54
„ acetate,	$\text{C}_4 \text{H}_8 \text{O}_2$	450·3	74
„ propionate,	$\text{C}_5 \text{H}_{10} \text{O}_2$	360·3	100
„ butyrate,	$\text{C}_6 \text{H}_{12} \text{O}_2$	297·0	119
„ valerate,	$\text{C}_7 \text{H}_{14} \text{O}_2$	241·3	134
Methyl acetate,	$\text{C}_3 \text{H}_6 \text{O}_2$	534·5	56
Ethyl „	$\text{C}_4 \text{H}_8 \text{O}_2$	450·3	74
Propyl „	$\text{C}_5 \text{H}_{10} \text{O}_2$	362·8	102
Butyl „	$\text{C}_6 \text{H}_{12} \text{O}_2$	305·3	116
Amyl „	$\text{C}_7 \text{H}_{14} \text{O}_2$	230·2	140

Formic and acetic acids, however, are exceptions to Guerot's law, which he attributes to possible impurities in the specimens examined. He also points out that the coefficients of capillarity of the ethers are very much higher than those of the alcohols or acids which give rise to them.

In the last column of the above Table I have added the boiling-points of the respective liquids; and it will be seen that, with the exception of acetic and formic acids, the coefficients of capillarity diminish as the boiling-point increases. Also, if we compare together an acid with the isomeric ethers, we find that the same rule holds good; thus:—

		Coefficients of capillarity.	Boiling-point.
{ Propionic acid,	$C_3 H_6 O_2 \dots$	189.0	141
{ Methyl acetate,	$C_3 H_6 O_2 \dots$	534.5	56
{ Ethyl formate,	$C_3 H_6 O_2 \dots$	542.0	54
{ Butyric acid,	$C_4 H_8 O_2 \dots$	129.5	162
{ Ethyl acetate,	$C_4 H_8 O_2 \dots$	450.3	74
{ Valerianic acid,	$C_5 H_{10} O_2 \dots$	92.3	175
{ Ethyl propionate,	$C_5 H_{10} O_2 \dots$	360.3	100
{ Propyl acetate,	$C_5 H_{10} O_2 \dots$	362.8	102
{ Caproic acid,	$C_6 H_{12} O_2 \dots$	64.0	205
{ Ethyl butyrate,	$C_6 H_{12} O_2 \dots$	297.0	119
{ Butyl acetate,	$C_6 H_{12} O_2 \dots$	305.3	116
{ Amyl acetate,	$C_7 H_{14} O_2 \dots$	230.2	140
{ Ethyl valerate,	$C_7 H_{14} O_2 \dots$	241.3	134

Passage of Gases through a Liquid Diaphragm.—Draper has shown that a soap-bubble immediately expands in size on being introduced into a vessel containing CO_2 , owing to the gas passing through the liquid diaphragm of soap more rapidly than the air passes out. Exner (Pogg. Ann. clv. pp. 321, 443) has made this observation the basis of a method for determining the rapidity with which gases diffuse through such liquid diaphragms; and he has found that $a = \frac{c}{\sqrt{d}}$, where a = rapidity of diffusion, c = coefficient of absorption of the gas, $d = \frac{\text{molecular weight of gas}}{2}$ = density of the gas. Thus:—

	c .	d (air = 1).	$\frac{c}{\sqrt{d}}$.	a , observed.	a , calculated.
N	.015	.97	.015	.86	.85
Air	.017	1.00	.017	1.00	1.00
O	.030	1.11	.028	1.95	1.60
Coal-gas...	.025	.48	.036	2.27	2.12
H	.019	.07	.072	3.77	3.89
C_2	1.002	1.52	.812	47.10	45.10
H_2S	3.165	1.17	2.94	165.00	163.30
NH_3	700	.59	903	46000.00	54450.00

Absorption of Gases by Charcoal.—Saussure and Hunter have determined the volumes of various gases which are absorbed by 1 volume of charcoal under given conditions. If we take their results and multiply the volume found by them by the respective densities of the various gases, we obtain the relative weights absorbed by 1 volume of charcoal; and on

comparing these weights together, we find, as a general rule (to which there are but three exceptions), that the higher the boiling-point of the gas when liquid, the greater is the weight absorbed by a given volume of charcoal.

Angus Smith (Chem. News, xxxix. p. 77) has recently remarked that the weight of several of the elementary gases absorbed by 1 volume of charcoal

$$= \frac{(\text{atomic weight})^2}{\text{quantivalence of the element}}.$$

Thus, if the weight of H absorbed be taken as the unit, then the weight of O absorbed under similar conditions

$$= 128 = \frac{16^2}{2},$$

and the weight of N absorbed

$$= 65.3 = \frac{14^2}{3}.$$

Further experiments in the case of other elementary gases, however, are required to confirm this interesting and curious relation.

Compressibility and Expansibility of Liquids.—According to Amagat (*Compt. Rend.* lxxxv. pp. 27, 139), the compressibility of compounds belonging to the same homologous series diminishes regularly as we ascend in the series, or as the boiling-point increases. The introduction of S, Cl, or Br into a compound tends to render it less compressible.

Kopp has compared together the rates of expansion (as determined by himself and Pierre) of nearly ninety liquids; and he finds, as a general rule (but one to which there are many exceptions), that the most expansible liquids are those which have the lowest boiling-points. In the case of compounds belonging to the same homologous series the rate of expansion *always* diminishes as we ascend the series, and therefore as the boiling-point increases. The fatty acids expand less rapidly than any of their ethers, at any rate up to the amylic ethers; all data are wanting for the higher ethers. The alcohols expand almost exactly at the same rate (though somewhat less rapidly) as the corresponding valerates. The rate of expansion also diminishes from chlorides to bromides, and thence to iodides, and therefore as the boiling-point increases; this also holds good, as far as is known, for the halogen compounds of the elements.

As regards gases, Mendeljeff and Kajander (*Deut. chem. Ges. Ber.* ix. p. 1311) have recently shown that gases having the same molecular weight have the same coefficient of expansion, but that this coefficient increases with the molecular

weight of the gas. This holds good in the case of H_2 , CO , N_2 , CO_2 , N_2O , and SO_2 . But, for reasons explained in the original paper, HBr and SiF_4 are exceptions to the rule.

Conducting-power of Gases for Heat.—Magnus and, more recently, Stefan (*Chem. Centr.* 1875, p. 529) have proved that gases differ in their conducting-power for heat; whilst Clausius (*Pogg. Ann.* cxv. p. 1) and Mohr (*Deut. chem. Ges. Ber.* iv. p. 85) have shown from mechanical considerations that these conducting-powers are proportional to the velocity of the molecules of the gas, and, further, that under equal pressure, this velocity is inversely proportional to the square root of the densities.

Absorbing-power for Heat.—Aymonnet (*Compt. Rend.* lxxxiii. p. 971) concludes from his researches that the coefficient of atomic absorption for heat is constant (1) for all substances dissolved in the same liquid, (2) for all substances of similar chemical constitution. The coefficient of atomic absorption = $\frac{AE}{DN}$, where A = coefficient of absorption, E = atomic weight, D = density, N = number of atoms. On comparing his conclusions, however, with his experimental numbers, they do not appear to be justified thereby to the extent one might have hoped; the above statements apply almost equally well to the coefficient of absorption itself as to the coefficient of atomic absorption. If one may judge from the few liquids submitted to experiment, the coefficient of absorption of compounds of analogous composition appears to be proportional to the boiling-point of the liquid reckoned from the absolute zero; thus:—

	Boiling-point reckoned from -273° C. (a).	Coefficient of absorption (c).	$\frac{a}{c}$
Methyl alcohol	339	·6879	·00202
Ethyl „	351	·6992	·00199
Amyl „	405	·7752	·00191
Benzene	354	·4866	·00137
Toluene	384	·4933	·00128
Turpentine	438	·5805	·00133

Latent Heat of Liquids.—In an important paper (*Compt. Rend.* lxxxii. p. 260) Pictet has shown:—(1) That the latent heat of all liquids brought to one and the same pressure, multiplied by the molecular weight, give a constant product. (2) For all liquids the difference of the internal latent heats at any two temperatures multiplied by the atomic weight is a

constant number. (3) The latent heats of all liquids are simple multiples of the specific heats.

The following are some of the more important relations which have been traced between the composition of substances and their action, or otherwise, on light.

Optical Rotation.—According to Pasteur (*Recherches*, p. 27), molecules are divisible into two classes:—(1) Those in which the atoms are symmetrically arranged; (2) those in which the arrangement is asymmetrical. The property of optical activity belongs to the latter class only; and Le Bel has proposed the hypothesis that when a C atom is united directly with four different elements or radicals, an asymmetrical form of molecule is produced, which must therefore be optically active. This view is supported by Van t'Hoff (*Bull. Soc. Chim.* [2] xxiii. p. 295), who finds, from the comparison of the chemical composition of all known active and many inactive bodies:—(1) That every organic combination which rotates the plane of polarized light contains an atom of asymmetrical carbon. The converse of this law, however, does not hold good. This may be attributed either to the presence of two isomerides of opposite rotatory power, or to the fact that asymmetrical carbon is not of itself sufficient to produce optical activity, which may also depend on the nature as well as on the mutual diversity of the atoms attached to the asymmetrical carbon atom. (2) Derivatives of optically active combinations lose their rotatory power when the asymmetry of their carbon atoms disappears. When the contrary happens, they do not generally lose it. These facts will in the future no doubt render great service in determining the constitution of optically active chemical compounds.

Specific Refraction.—Gladstone and Dale (*Journ. Chem. Soc.* [2] viii. pp. 101, 147; x. p. 1), Landolt (*Pogg. Ann.* cxxiii. p. 603), and others have shown that there is a true connexion between chemical composition of compounds and their molecular refractive energy ($=m \times \frac{n-1}{d}$, where m = molecular weight, n = refractive index, and d = specific gravity).

The following are the more important points in regard to this subject:—(1) Isomerides and metamerides, if of similar chemical constitution, have the same molecular refractive energy. (2) The molecular refractive energy increases in homologous series by 7.6 for each increase of CH_2 . (3) By the addition of H_2 , the molecular refractive energy increases by 2.6. (4) The molecular refractive energy of the acids $\text{C}_n\text{H}_{2n}\text{O}_2 = 7.6n + 6$. (5) The atomic refractive energy of C, H, and O are therefore $\text{C} = 5$, $\text{H} = 1.3$, $\text{O} = 3$. (6) From this

it follows that the molecular refractive energy of a compound is equal to the sum of the atomic refractive energies of its components; thus :—

Molecular refractive energy.		
Calculated.		Found
		$= \left(\frac{n-1}{d} \times m \right).$
$C_4 H_{10} O = (4 \times 5) + (10 \times 1.3) + 3$	$= 36.0$	36.3
$C_2 H_4 O_2 = (2 \times 5) + (4 \times 1.3) + (2 \times 3)$	$= 21.2$	21.2

Similar relations also hold good in regard to compounds of the aromatic series; but their molecular refractive energies are higher than those calculated from the values for C, H, and O given above. Landolt (*Chem. Centr.* 1872, p. 705) has shown, however, that a constant refraction-equivalent is obtained for the benzene nucleus in its various derivatives when the refraction-equivalents taken for the C, H, and O of the side chains are those given above in the case of the fatty series.

Gladstone (*Phil. Trans.* 1870, p. 9) and Haagen (*Pogg. Ann.* cxxxi. p. 117) have also, by a similar method, calculated the refraction-equivalents of a large number of elements, and have shown that, as a general rule, each element retains its own refractive power with whatever other elements it may be combined, and also that the specific refractive energies are, for the most part, greater as their combining-powers are less. Lothar Meyer (*Modernen Theorien der Chemie*) has likewise pointed out that the atomic refractive energies, or refraction-equivalents, of the elements are a periodic function of their atomic weights.

Photographic Transparency.—As regards the photographic transparency or diactinic power of inorganic substances, Miller (*Phil. Trans.* 1863, p. 1; *Chem. Soc. Journ.* xviii. p. 59) found that fluorides are chemically the most transparent, after which come the chlorides of the alkalies and alkaline earths, then the bromides, and finally the iodides. Here the diactinic power diminishes with the atomic weight of the halogen. The nitrates, as a class, possess a remarkably high absorptive power for chemical rays, whereas the reverse is the case as regards the chlorates.

Absorption of the Ultra-violet Rays of the Spectrum by Organic Substances.—This subject has been investigated by Hartley and Huntington (*Nature*, xix. p. 259), who find :—(1) The normal alcohols $C_n H_{2n+1} OH$ are remarkable for transparency to the ultra-violet rays of the spectrum. (2) The normal fatty acids show a greater absorption of the more refrangible rays

of the ultra-violet spectrum than the normal alcohols containing the same number of carbon atoms. (3) There is an increased absorption of the more refrangible rays for each increase of CH_2 in the molecule of the alcohols and acids. (4) Like alcohols and acids, the ethereal salts derived from them are highly transparent to the ultra-violet rays, and do not exhibit absorption-bands. (5) Aromatic hydrocarbons, phenols, acids, and amines derived therefrom are remarkable for their powerful absorption of the ultra-violet rays. (6) Isomeric bodies containing the benzene nucleus exhibit widely different spectra, both as regards the position and intensity of the absorption-bands; and so much is this the case that these absorption-spectra may be used as a means of identifying organic substances, and is a most delicate test of their purity.

Relative Sensitiveness of Photographic Papers.—MacDougall (Chem. Journ. [2] iii. p. 183) has shown that the relative sensitiveness of silvered papers salted with solutions of the haloid salts of various metals is independent of the nature of the metal with which the halogen is combined, and depends solely on the quantity and nature of the halogen. Combining the results of Wright (Chem. Journ. [2] iv. p. 33) and of Carey Lea (Am. Journ. Sci. [3] ix. p. 269), we find that the photographic sensitiveness increases with the atomic weight of the halogen. This, however, does not hold good for mixtures of the haloid salts.

Spectra of the Elements.—Several cases have been observed which appear to show that the atomic weights of the elements have a marked and definite influence on the nature of the spectra which they yield; thus:—

Chlorine Group (Cl, Br, I).—(1) In passing from Cl to Br, and thence to I, the spectra expand gradually towards the ultra-violet. (2) Each spectrum has two maxima of light, which approach each other as the atomic weight of the element increases; and the brilliant lines seem also to become broader, and the maxima move towards the violet, as we approach I (Ditte, *Compt. Rend.* 1871, lxxiii. p. 738).

Ditte also examined the spectrum of fluorine produced by passing the induction-spark through SiF_4 , and found that though the spectrum had two well-defined maxima, yet it did not present the brilliant region peculiar to the spectrum of the Cl group, and the maxima are wide apart. He therefore concludes that, according to its spectrum, F ought not to be placed among the halogens. Nevertheless I think it resembles the Cl group in possessing two maxima, which are wider apart than in the case of the other elements of the group,

because the atomic weight of F is less than that of Cl; but in other respects the spectrum is very different from those of Cl, Br, and I. These facts, it appears to me, agree perfectly well with Mendeljeff's theory of odd and even members of a group (*vide supra*); for Cl, Br, and I are odd members, whilst F is an even member of the halogen group of elements.

Sulphur Group (S, Se, Te). Ditte (*Compt. Rend.* lxxiii. p. 622) has also examined the spectra of this group by passing the induction-spark through the vapours of their chlorides and eliminating the lines due to Cl; and he found:—(1) That the spectrum of S extends between the divisions 22 and 120 on the micrometer. It shows two maxima, one being a double line in the yellow, and the other in the commencement of the blue. (2) The spectrum of Se begins at 19 and ends at 125, and also shows two maxima, the first between the yellow and green, and the second in the blue. (3) The spectrum of Te begins at 18 and ends at 146, and also shows two maxima, one in the indigo and the other in the violet.

The spectra of this group thus increase in range, especially towards the ultra-violet end, as we pass from S to Te, *i. e.* with the atomic weight. Each of the spectra likewise shows two maxima, one being superior to the other; and as we pass from S to Te, the two maxima move towards the violet end.

Phosphorus Group (P, As, Sb, Br).—Ditte (*Compt. Rend.* lxxiii. p. 738) has likewise found:—(1) That the spectra of the chlorides of the elements belonging to the phosphorus group expand more and more, especially towards the violet end, as the atomic weight of the positive element increases, or in proportion as the properties of the element approach to those of the metals. (2) The spectra of the chlorides of this group all have three maxima of luminous intensity, due to very brilliant lines separated by comparatively dark intervals. These three maxima move towards the violet end as the atomic weight of the positive element increases.

Carbon and Silicon Group (C, Si, Ti, Zr only were examined).—The spectra of this group have been investigated by Troost and Hautefeuille (*Compt. Rend.* lxxiii. p. 620) by passing the induction-spark through atmospheres of the chlorides and eliminating the lines due to Cl. They found:—(1) That as we pass from C to Si, and thence to Ti and Zr, *i. e.* as the atomic weight increases, the rays become more and more refrangible. (2) The spectra of each element has three maxima, caused by groups of luminous bands. (3) As the atomic weight of the element increases, these three maxima advance more and more towards the violet. Lead also, though not examined by Troost

and Hautefeuille, appears to be in accordance with the above rules, giving a more refrangible spectrum than Zr.

Aluminium Group (Al, Ga, In, Tl).—*Aluminium* is characterized by the large number of bright lines lying close together, of which the most important in the red are those of wave-lengths 6243 and 6245, and in the blue 4661 and 4662. The aluminium-bands seen in the ultra-violet are extremely characteristic.

Gallium. This spectrum consists of two violet lines, of which the brighter Ga α , of wave-length 4170, lies a little more towards the blue than In β , and the second, Ga β (wave-length 4031), a little more towards the violet than K β . *Indium*. The spectrum of this metal is made up of an intense indigo-blue line In α (wave-length 4509) and a less intense violet line In β , 4101. *Thallium* is characterized by the intense green line Tl α (wave-length 5349). From the above it is seen that, as the atomic weight increases, the rays become less and less refrangible: thus Al, the metal with lowest atomic weight, is remarkable for bright lines in the ultra-violet, whereas Tl, with the greatest atomic weight, is recognized by its well-known green line.

Calcium Group (Ca, Sr, Ba).—Here also the rays towards the violet end of the spectrum become less and less refrangible as the atomic weight increases.

Magnesium Group (Be, Mg, Zn, Cd, Hg).—In the case of these elements the rays towards the violet end tend, on the other hand, to become more and more refrangible as the atomic weight increases. Zinc, however, is an exception to this rule.

Potassium Group (Li, Na, K, Rb, Cs).—The spectra of this group become less refrangible from K to Cs, *i. e.* as the atomic weight increases. In the case of Na and Li, which are exceptions, may they not give lines in the ultra-violet beyond K β which have not yet been observed?

Copper Group (Cu, Ag, Au).—Here the rays towards the violet end become less and less refrangible as the atomic weight increases.

The general conclusions, therefore, which are to be drawn from the above remarks on the spectra of the elements are as follows:—

(1) That for non-metals and quasimetals (including the Cl, S, P, and C groups) the spectra become more and more refrangible as the atomic weight increases.

(2) That for true metals (Al, Ca, K, and Cu groups) the reverse holds good, the spectra becoming less and less refrangible as the atomic weight increases. The Mg group, and

also Li and Na, are exceptions to the latter of these statements*.

Spectra of Chemical Compounds.—Lockyer (Proc. Roy. Soc. 1873, p. 285) has investigated the spectra of metals when combined with various negative elements, and finds that regular relations obtain. Thus in the case of PbF_2 , PbCl_2 , PbBr_2 , and PbI_2 it appears that the length and number of the metallic lines observed diminish as the atomic weight of the non-metallic element increases. This was also found to hold good with the haloid compounds of the metals of the alkalies and alkaline earths.

Mitscherlich (Pogg. Ann. cxxi. p. 459; Phil. Mag. [4] xxviii. p. 169) has also pointed out a singular relation between the atomic weights of the haloid compounds of Ca, Sr, and Ba and the distance between the chief lines of their spectra. He finds that in the case of Ba, the distances between the most prominent lines given by the Ba haloid compounds respectively are directly proportional to the molecular weights of these compounds, whilst for the haloid compounds of Sr and Ca these distances are inversely proportional to the molecular weights of the compounds.

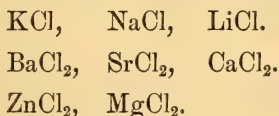
Colour and Tinctorial Properties of Carbon Compounds.—All organic colouring-matters are nearly related in composition. They all belong to the aromatic group of compounds, and, on reduction, become colourless, owing to combination with hydrogen. All quinones and azo-compounds are coloured; and they all combine with nascent hydrogen and become colourless. In these bodies, too, the O or N is combined in a manner different to what they are in other compounds. Thus we find that all bodies (quinones) which contain O united in the following manner, $\left(\begin{smallmatrix} \text{O} \\ -\text{O}- \end{smallmatrix}\right)$, are coloured, and combine with H, thus: $\left(\begin{smallmatrix} -\text{OH} \\ -\text{OH} \end{smallmatrix}\right)$, forming colourless compounds (hydroquinones). We may also obtain similar colourless bodies by using monatomic alcohol-radicals instead of H. In azo-compounds we have the N combined thus, $(-\text{N}=\text{N}-)$; and they also combine with H to form colourless

* Since the above was in proof, I have noticed a paper by Ciamician (*Wien. Akad. Ber.* lxxvi. p. 429; Chem. Soc. Journ. Abstracts, Sept. 1879) in which, from a study of the spectra of thirty-one elements, he concludes "that the spectra of chemically analogous elements correspond either singly or in groups, and that therefore every group of elements has a spectrum which only differs from those of the various members of the group in that the homologous lines or groups of lines are transposed either in one direction or another"—and, further, that the properties of the elements are a function, not only of their atomic weights, but also of the wavelengths of their respective lines.

compounds, $\left(\begin{array}{cc} \text{---N---N---} \\ | \quad | \\ \text{H} \quad \text{H} \end{array} \right)$. The nitro-compounds of the phenols also always have a slight colour, the N and O being united thus, $\left(\text{---N} \begin{array}{c} \text{O} \\ \diagup \quad \diagdown \\ \text{O} \end{array} \right)$; and on reduction this becomes $\left(\text{---N} \begin{array}{c} \text{H} \\ \diagup \quad \diagdown \\ \text{H} \end{array} \right)$,

as in the important series of anilines. (Notes of Prof. Schorlemmer's lectures on Organic Chemistry, 1871.)

Electric Conductivity.—Kohlrausch and Grotian, and also Tollinger, have found that the conducting-power of the following chlorides when in solution are in the order represented below, beginning with the highest in each class:—



The order of conductivity of the sulphates also appears to be the same as above. We thus see that the conductivity of the salts in the same group, when in solution, diminishes with the atomic weight of the positive element.

Magnetic and Diamagnetic Powers.—It has been noticed (Watts's Dict. iii. p. 773) that those metals which are highly diamagnetic possess large atomic volumes, whereas those which are highly magnetic have but small atomic volumes. Thus Bi, the most diamagnetic substance known, has the high atomic volume 21.2, whilst Fe, Ni, and Co, which are highly magnetic, have very low atomic volumes, viz. about 3.5. The relative distances between the atoms, however, are not the only cause which conditions the magnetism or diamagnetism of a body; for there are several well-known exceptions to the above rule, as K and Na, which have large atomic volumes, and are slightly magnetic, while Cu and Zn, on the other hand, have small atomic volumes, and are slightly diamagnetic.* For elements belonging to the same group, the diamagnetism appears to increase with the atomic weight: thus

* In a paper which will be published shortly in the *Ber. deut. chem. Ges.*, I have shown that those elements which belong to Mendeljeff's *even* series (vide *suprà*) are *magnetic*, whilst those which belong to *odd* series are diamagnetic. Out of about forty elements to which this rule may be applied there is not a single exception, unless it be Na—which Faraday found to be diamagnetic, and therefore in accordance with the rule, whereas Lamy states that it is first repelled and afterwards attracted by a magnet.

Specific Magnetism.

O	= + 0.181	(Faraday),
S	= - 1.14	(Becquerel),
Se	= - 1.65	„
N	= + 0.003	(Faraday),
P	= - 1.64	(Becquerel),
Sb	= - 4.10	„
Bi	= - 22.67	„
H	= - 0.001	(Faraday),
Cu	= - 1.68	„
Ag	= - 2.32	„
Au	= - 3.47	„

Physiological Action.—The results obtained by Gamgee, as regards the physiological action of ortho-, pyro-, and metaphosphoric acids, and also of the salts of vanadic acid, appear to show that there is likewise some regular connexion between chemical constitution and physiological action.

Chemical Affinity. Mutual Displacement of Halogens.—Potilizin (*Chem. Centr.* 1876, p. 577) has determined the amount of Cl displaced by Br when the latter is allowed to act upon metallic chlorides; and he found that the amount of Cl replaced by Br was proportional to the atomic weights of the metals with which the Cl was combined, provided they had the same atomicity in the respective compounds: thus

Per cent. of Cl replaced by Br in:—

Group I.	{ NaCl	KCl	AgCl		
	{ 7.16	12.54	33.34		
Group II.	{ CaCl ₂	SrCl ₂	BaCl ₂	HgCl ₂	PbCl ₂
	{ 3.2	6.64	9.86	15.16	15.64

Gustavson (*Ann. Chim. Phys.* (5) ii. p. 200; Watts's Dict. viii. p. 429) has also determined the amount of mutual replacement which occurs when CCl₄ or CBr₄ acts upon various other bromides or chlorides. The following Table represents the percentage of the double decomposition which takes place when the several pairs of bodies are heated together in sealed tubes to 150°–200°:—

Mixture.	Percentage of decomposition.
$\left\{ \begin{array}{l} 4 \text{ BCl}_3 + 3 \text{ CBr}_4 \\ 4 \text{ BBr}_3 + 3 \text{ CCl}_4 \end{array} \right\}$	$\left\{ \begin{array}{l} 10\cdot12 \\ 89\cdot97 \end{array} \right\}$
$\left\{ \begin{array}{l} \text{SiCl}_4 + \text{CBr}_4 \\ \text{SiBr}_4 + \text{CCl}_4 \end{array} \right\}$	$\left\{ \begin{array}{l} 12\cdot46 \\ 87\cdot54 \end{array} \right\}$
$\left\{ \begin{array}{l} \text{TiCl}_4 + \text{CBr}_4 \\ \text{TiBr}_4 + \text{CCl}_4 \end{array} \right\}$	$\left\{ \begin{array}{l} 43\cdot61 \\ 57\cdot39 \end{array} \right\}$
$\left\{ \begin{array}{l} 4 \text{ AsCl}_3 + 3 \text{ CBr}_4 \\ 4 \text{ AsBr}_3 + 3 \text{ CCl}_4 \end{array} \right\}$	$\left\{ \begin{array}{l} 71\cdot78 \\ 28\cdot91 \end{array} \right\}$
$\left\{ \begin{array}{l} \text{SnCl}_4 + \text{CBr}_4 \\ \text{SnBr}_4 + \text{CCl}_4 \end{array} \right\}$	$\left\{ \begin{array}{l} 97\cdot52 \\ 2\cdot16 \end{array} \right\}$

From this it is seen that the higher the atomic weight of the element (B, Si, Ti, As, Sn) united with Cl, the more is the Cl replaced by the Br of the CBr_4 ; and the higher the atomic weight of the element united with Br, the less will that Br be replaced by the Cl of the CCl_4 .

Cross and Suguira (Chem. Soc. Journ. 1878, p. 405) have shown that the amount of O in PbO replaced by the action of the halogens at a high temperature is the greater the lower the atomic weight of the halogen.

Mills (Phil. Mag. [4] xl. p. 134; xlv. p. 343), from his experiments on the action of POCl_3 on various nitrates, has calculated the ratio (a) between the amount of O and Cl left in the respective residues after distilling off the excess of POCl_3 , and finds that, with one exception, this ratio increases with the equivalent weight of the nitrate: thus

	$a \times \frac{\text{P}_2\text{O}_5}{\text{Cl}}$	Equivalent weight.
TiNO_3	8·76	265·3
AgNO_3	5·48	169·9
$\frac{1}{2}\text{Pb}(\text{NO}_3)_2$	5·17	165·6
RbNO_3	2·38	147·4
CsNO_3	2·21 (?)	195·0
KNO_3	1·99	101·1
NaNO_3	1·70	85·1
LiNO_3	1·61	69·0

The Rate of Etherification of Alcohols by Acetic Acid has been determined by Menschutkin (*Ber. deut. chem. Ges.* x. p. 1728; xi. p. 1507); and he finds that the relative rate of etherification diminishes as the molecular weight of the alcohol increases: thus

	Relative rate of etherification.	Limit of etherification.
Methyl alcohol...	—	69·5
Ethyl „ ...	70·5	66·6
Propyl „ ...	70·2	66·8
Butyl „ ...	69·6	67·3
Isobutyl „ ...	66·6	67·4
Octyl „ ...	64·4	72·3
Cetyl „ ...	—	80·4

A similar relation was also observed as regards the etherification of secondary alcohols and phenols (*Ber. deut. chem. Ges.* xi. pp. 2117, 2148).

The limit of etherification of the alcohols of the paraffin series increases with the molecular weight, except in the case of methyl alcohol.

In the preceding pages I have endeavoured to trace and place in a connected form the more important relations between the atomic weights or chemical composition and the various chemical and physical properties of the elements, and more especially of their compounds. In order that this connexion might be rendered the more evident, I have limited myself in great part to those properties which are capable of being represented numerically, and consequently to what are generally termed the physical properties.

The relations between atomic, molecular, or equivalent weights and specific heats (Dulong and Petit's law), electrolysis (Faraday's laws), and diffusion of gases (Graham's law) are so well known that I have thought it unnecessary to refer to them in detail.

With one exception, too, I have omitted all mention of the connexion which undoubtedly exists between chemical composition and the thermal phenomena of chemical reactions. I reserve the discussion of this interesting subject to a subsequent communication; for the limits of the present paper would not admit of full justice being done to what I believe to be one of the most important points from which to view chemical decomposition, and which promises in the future to throw most light on the more vexed problems of chemical research.

LVI. *Investigations in Optics, with special reference to the Spectroscope.* By LORD RAYLEIGH, F.R.S.

[Continued from p. 411.]

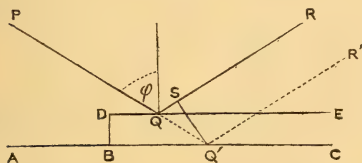
§ 5. *On the Accuracy required in Optical Surfaces.*

FOUCAULT, in the memoir already referred to, was, I believe, the first to show that the errors of optical surfaces should not exceed a moderate fraction of the wave-length of light. In the case of perpendicular reflection from mirrors, the results of § 4 lead to the conclusion that no considerable area of the surface should deviate from truth by more than one eighth of the wave-length. For a glass surface refracting at nearly perpendicular incidence the admissible error is about four times as great. It will be understood, of course, that the errors of one surface in an optical train may compensate for those of another, all that is necessary being that the resultant error of retardation rise nowhere to importance.

In the case of oblique reflection at an angle ϕ , the error of retardation due to an elevation BD (fig. 7) is

$$\begin{aligned} QQ' - QS &= BD \sec \phi (1 - \cos SQQ') \\ &= BD \sec \phi (1 + \cos 2\phi) = 2 BD \cos \phi, \end{aligned}$$

Fig. 7.

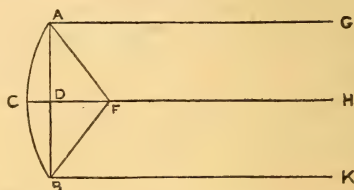


from which it follows that an error of given magnitude in the figure of a surface is less important in oblique than in perpendicular reflection. At first sight this result appears to be contradicted by experience; for it is well known to practical opticians that it is more difficult to secure a satisfactory performance when reflection is oblique. The discrepancy is explained in great measure when we take into account the kind of error to which surfaces are most liable. No important deviation from a symmetrical form is to be feared; but a surface intended to be plane may easily assume a slight general convexity or concavity. Now in direct reflection, a small curvature is readily and almost completely compensated by a small motion of the eyepiece giving a change of focus; but the compensation obtainable in this way is much less perfect when the reflection is oblique. In the first case the family of surfaces

approximating to a plane, which will answer the purpose, coincides with the family of surfaces most likely to be produced; in the second case the family of ellipsoidal or hyperbolic surfaces capable (with suitable focus) of giving good definition contains only one symmetrical member—the perfect plane. In order to test experimentally the correctness of the theoretical result, it would be necessary to retain the focus suitable to the true surface, and not to allow a readjustment by which its errors may be in greater or less degree compensated.

A further difficulty, not touched by the preceding considerations, still remains to be mentioned. In the ordinary method of testing plane surfaces by measuring the change of focus required when a distant point is viewed through a telescope, first directly, and then after reflection in the surface, the test is found to be more delicate as the reflection is more oblique. The explanation of the apparent inconsistency will be best understood by a calculation of the focal length of mirrors, founded directly upon the principles of the wave theory. Let $A C B$ (fig. 8) be an arc of a (parabolic) mirror, which reflects parallel rays $G A$, $H D$, $K B$

Fig. 8.



to a focus F . $A D = y$, $D F = f$, $C D = t$. In calculating the retardations of the various rays, we will take as standard the phase at F of a ray coincident with $H D$, reflected at D (as by a plane mirror $A D B$) instead of at C , so that the actual retardation at F of the central ray $H C F$ is $2t$. The retardation of the extreme ray $G A F$ is $A F - F D$, or $\sqrt{(f^2 + y^2)} - f$. Since F is by supposition an optical focus, the phases of all the rays must be the same, and thus

$$\sqrt{(f^2 + y^2)} - f = 2t. \quad . \quad . \quad . \quad . \quad (1)$$

If the aperture ($2y$) be small in proportion to the focal length,

$$\sqrt{(f^2 + y^2)} - f = \frac{1}{2} \frac{y^2}{f} \text{ approximately,}$$

so that

$$f = \frac{y^2}{4t}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the limit it is a matter of indifference whether f be measured from D or from C . If r be the radius of curvature at C ,

$$t = \frac{1}{2} \frac{y^2}{r},$$

and

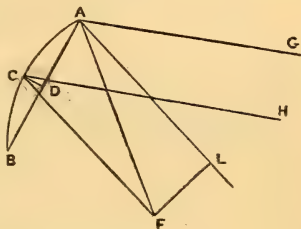
$$f = \frac{1}{2}r, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

the usual formula.

When the incidence is oblique, there are different foci in the primary and the secondary planes. Considering first the case

Fig. 9.

of the primary plane, let $A C B$ (fig. 9) be the mirror, and F the focus. $AD=y$, $CD=t$, $HCD=\phi$. AL is the course which the ray GA would take if reflected by the plane surface ADB . C is that point of the mirror at which the tangent is parallel to AB . The retardation at F of the ray GAF is $AF-AL$; and the retardation of the ray HCF due to the curvature of the mirror is $2t \cos \phi$. These retardations must be equal; and thus



$$AF - AL = \sqrt{(AL^2 + FL^2)} - AL = 2t \cos \phi.$$

When the aperture is small, $\sqrt{(AL^2 + FL^2)} - AL$ is approximately $\frac{1}{2} \frac{FL^2}{AL}$; ultimately AL may be identified with f_1 , the focal length in the primary plane, and FL may be identified with $y \cos \phi$, so that

$$\frac{y^2 \cos^2 \phi}{2f_1} = 2t \cos \phi, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or

$$f_1 = \frac{y^2 \cos \phi}{4t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Thus it appears that, so far as the primary focal length is concerned, the diminished retardation of the central ray due to obliquity is outweighed by the corresponding diminution of effective aperture (FL); but although in consequence of obliquity a greater change of focus (estimated from that required for the plane surface) is necessary in order to get the best result, still, if no change of focus be admitted, the error due to curvature is less sensible in oblique than in direct reflection.

The preceding discussion assumes that the same extent of surface is used in all cases. In testing planes by reflection it often happens that a *greater* extent of surface is used in the case of obliquity, the field being limited by the object-glass of the telescope rather than by the reflecting plane. Under such circumstances the loss of definition (with focus unaltered) due to curvature is aggravated by obliquity.

In the secondary focal plane there is no diminution of effective aperture due to obliquity. Instead of (4) we have

$$\frac{y^2}{2f_2} = 2t \cos \phi, \quad \dots \dots \dots (6)$$

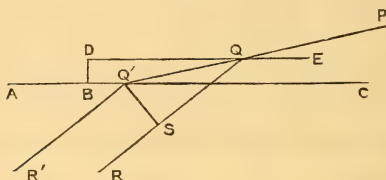
or

$$f_2 = \frac{y^2}{4t \cos \phi} \dots \dots \dots (7)$$

In this case the favourable effect of obliquity shows itself directly in the increased value of f_2 .

We will now consider the effect of errors in a refracting

Fig. 10.



surface. The error of retardation due to an elevation BD is $\mu QS - QQ' = QQ' \{ \mu \cos (\phi - \phi') - 1 \}$

$$= BD \frac{\mu \cos \phi \cos \phi' + \mu \sin \phi \sin \phi' - 1}{\cos \phi}$$

$$= BD \frac{\mu \cos \phi \cos \phi' - (1 - \sin^2 \phi)}{\cos \phi} = BD (\mu \cos \phi' - \cos \phi),$$

since

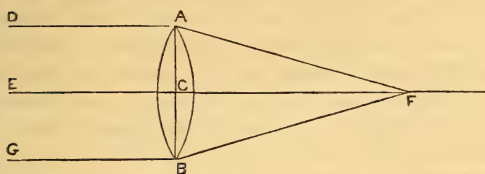
$$\mu \sin \phi' = \sin \phi.$$

As a function of obliquity $\mu \cos \phi' - \cos \phi$ is least $(\mu - 1)$ when the obliquity is zero; it is greatest $\sqrt{(\mu^2 - 1)}$ when the obliquity is 90° . Thus the retardation for a given error of elevation *increases* somewhat with the obliquity, being in the case of glass about twice as great at a grazing as at a perpendicular incidence.

Before concluding this section, it may be worth while to point out how the principles of the wave theory may be applied directly to calculate the focal length of lenses. The relative retardations of the rays D A F and E C F (fig. 11) are evidently A F - C F and $(\mu - 1)t$, if t denote the thickness of the lens at the centre. Thus, if A C = y , F C = f ,

$$(\mu - 1)t = \sqrt{(f^2 + y^2)} - f = \frac{1}{2} \frac{y^2}{f} \dots \dots \dots (8)$$

Fig. 11.



approximately. For glass $\mu - 1 = \frac{1}{2}$ nearly; so that the old rule*, that "in glass lenses the half-breadth is a mean proportional between the thickness and the focal length," is more scientific than the usual formula in terms of the radii of curvature. If the lens do not terminate in a sharp edge, we may take as the effective thickness the difference of the thicknesses at the centre and at the edge.

For an oblique central pencil, the focal lengths in the two principal planes may be obtained as in the case of the mirror. They take the form

$$\frac{y^2 \cos^2 \phi}{2f_1} = (\mu \cos \phi' - \cos \phi)t, \quad . \quad . \quad . \quad (9)$$

$$\frac{y^2}{2f_2} = (\mu \cos \phi' - \cos \phi)t, \quad . \quad . \quad . \quad (10)$$

in which, if we please, we may substitute for t its value in terms of the radii of curvature, viz.

$$\frac{1}{2}y^2 \left(\frac{1}{r} + \frac{1}{s} \right).$$

§ 6. *The Aberration of Oblique Pencils.*

In treatises on geometrical optics it is usual to calculate the aberrations of mirrors and lenses for direct pencils, but in the case of oblique pencils to rest satisfied with determining the primary and secondary focal lengths. For most purposes indeed astigmatism is a worse defect than aberration, so that in the presence of the former it is not worth while to consider the latter; but in this respect the spectroscope is an exception, and the completion of its theory requires the consideration of the aberration of oblique pencils.

The reason of this peculiarity is not difficult to see. When a luminous point is observed through an optical instrument affected with astigmatism, there are three notable representa-

* Coddington's 'Optics,' p. 96.

tions of it to be obtained by varying the focus. At the primary and secondary foci the point is represented by perpendicular lines of small width, and at a particular intermediate position by a circle of light, called the circle of least confusion. In most cases the last representation would be the best; but if the object under examination be itself a uniformly luminous line parallel to one or other of the focal lines, the best result will evidently be obtained at the corresponding focus. Under these circumstances the image is not prejudiced by the astigmatism, and its perfection depends upon the amount of aberration. In the case of a properly adjusted spectroscope the slit is parallel to the edges of the prisms, and the spectrum is seen with best definition at the *primary* focus.

The aberration that we have now to consider is of higher order than that which affects symmetrical pencils, and therefore, when it occurs, is presumably of greater importance. Before calculating its amount in particular cases, it will be convenient to consider the general character of the effects produced by it. The axis of the pencil being taken as axis of z , let the equation of the wave-surface, to which all rays are normal, be

$$z = \frac{x^2}{2\rho} + \frac{y^4}{2\rho'} + \alpha x^3 + \beta x^2 y + \gamma x y^2 + \delta y^3 + \dots \quad (1)$$

The principal focal lengths are ρ and ρ' . In the case of symmetry ρ and ρ' are equal, and the coefficients of the terms of the third order vanish. The aberration then depends upon terms of the fourth order; and even these are made to vanish in the formulæ for the object-glasses of telescopes by the selection of suitable curvatures. If this be effected, the outstanding aberration will be of the sixth order; whereas in the case of unsymmetrical pencils, even if we should succeed in destroying the terms of the third order, there will still remain an aberration of the fourth order. It follows that every effort should be made to retain symmetry about the axis; but in the case of the spectroscope this is usually impossible. If we could secure a perfect parallelism of the incident light, and perfectly flat faces for our prisms, we should indeed get rid of aberration, and at the same time render ourselves independent of the adjustments of the spectroscope; for it is evident that no repetition of refractions at plane surfaces, however situated, could disturb the original parallelism of the light. The fact that most large spectroscopes are more or less sensitive to maladjustment of the prisms proves either that the faces are not flat, or that it is difficult to obtain a sufficiently accurate adjustment of the collimator. We shall suppose that the faces

of the prisms are surfaces of revolution, so that it is possible by proper adjustments to render every thing symmetrical with respect to a plane bisecting at right angles the refracting edges. If ρ be the primary focal length, this plane is that represented by $y=0$, and the equation of the wave-surface reduces to

$$z = \frac{x^2}{2\rho} + \frac{y^2}{2\rho'} + \alpha x^3 + \gamma xy^2, \quad \dots \quad (2)$$

terms of higher order being omitted.

The constants α and γ in (2) may be interpreted in terms of the differential coefficients of the principal radii of curvature. By the usual formula, the radius of curvature at the point x of the intersection of (2) with the plane $y=0$ is approximately $\rho(1-6\alpha\rho x)$. Since $y=0$ is a principal plane throughout, this radius of curvature is a principal radius of the surface; so that, denoting it by ρ , we have

$$\alpha = -\frac{1}{6\rho^2} \frac{d\rho}{dx}. \quad \dots \quad (3)$$

In the neighbourhood of the origin the approximate value of the product of the principal curvatures is

$$\frac{1}{\rho\rho'} + \frac{6\alpha x}{\rho'} + \frac{2\gamma x}{\rho}.$$

Thus

$$\delta\left(\frac{1}{\rho\rho'}\right) = -\frac{\delta\rho}{\rho^2\rho'} - \frac{\delta\rho'}{\rho'^2\rho} = \frac{6\alpha x}{\rho'} + \frac{2\gamma x}{\rho};$$

whence by (3),

$$\gamma = -\frac{1}{2\rho'^2} \frac{d\rho'}{dx}. \quad \dots \quad (4)$$

The equation of the normal at the point x, y, z is

$$z - \zeta = \frac{\xi - x}{\rho^{-1}x + 3\alpha x^2 + \gamma y^2} = \frac{\eta - y}{\rho'^{-1}y + 2\gamma xy}; \quad \dots \quad (5)$$

and its intersection with the plane $\zeta=\rho$ occurs at the point determined approximately by

$$\left. \begin{aligned} \xi &= -\rho(3\alpha x^2 + \gamma y^2), \\ \eta &= \frac{\rho' - \rho}{\rho'} - 2\rho\gamma xy, \end{aligned} \right\} \quad \dots \quad (6)$$

terms of the third order being omitted.

According to geometrical optics, the thickness of the image of a luminous line at the primary focus is determined by the extreme value of ξ ; and for good definition in the spectrocope

it is necessary to reduce this thickness as much as possible. One way of attaining this result would be to narrow the aperture; but, as we have seen in preceding sections, to narrow the horizontal aperture is really to throw away the peculiar advantage of large instruments. The same objection, however, does not apply to narrowing the *vertical* aperture; and in many spectroscopes a great improvement in definition may be thus secured. In general it is necessary that *both* γ and α be small. Since the value of ξ is independent of ρ' , it would seem that in respect of definition there is no advantage in avoiding astigmatism.

We will now examine more closely the character of the image at the primary focus in the case of a pencil of circular section. Unless $\rho' = \rho$, the second term in the value of η may be neglected. The rays for which $x^2 + y^2 = r^2$ intersect the plane $\zeta = \rho$ in the parabola

$$\frac{\rho\rho'^2(3\alpha - \gamma)}{(\rho' - \rho)^2} \eta^2 - \xi = 3\alpha\rho r^2; \quad . \quad . \quad . \quad (7)$$

and the various parabolas corresponding to different values of r differ from one another only in being shifted along the axis of ξ . To find out how much of the parabolic arcs are included, we observe that for any given value of r the value of η is greatest when $x = 0$. Hence the rays starting in the secondary plane give the remainder of the boundary of the image. Its equation, formed from (6) after putting $x = 0$, is

$$\eta^2 = - \frac{(\rho' - \rho)^2}{\rho\rho'^2\gamma} \xi, \quad . \quad . \quad . \quad . \quad (8)$$

and represents a parabola touching the axis of η at the origin. The whole of the image is included between this parabola and the parabola of form (7) corresponding to the maximum value of r .

The width of the image when $\eta = 0$ is $3\alpha\rho r^2$, and vanishes when $\alpha = 0$, *i. e.* when there is no aberration for rays in the primary plane. In this case the two parabolic boundaries coincide, and the image is reduced to a linear arc. If further $\gamma = 0$, this arc becomes *straight*, and then the image of a luminous line is perfect (to this order of approximation) at the primary focus. In general if $\gamma = 0$, the parabola (7) reduces to the straight line $\eta = 0$; that is to say, the rays which start in the secondary plane remain in that plane.

We will now consider the image formed at the secondary focus. Putting $\zeta = \rho'$ in (5), we obtain

$$\left. \begin{aligned} \xi &= \frac{\rho - \rho'}{\rho} x, \\ \eta &= -2\gamma\rho'xy. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (9)$$

If $\gamma = 0$, the secondary focal line is formed without aberration, but not otherwise. In general the curve traced out by the rays for which $x^2 + y^2 = r^2$, is

$$\left(\frac{\rho}{\rho - \rho'}\right)^2 \xi^2 + \frac{(\rho - \rho')^2 \eta^2}{4\gamma^2 \rho^2 \rho'^2 \xi^2} = r^2, \quad . \quad . \quad . \quad (10)$$

in the form of a figure of eight symmetrical with respect to both the axes. The rays starting either in the primary or in the secondary plane pass through the axis of ξ , the thickness of the image being due to the rays for which $x = y = r \div \sqrt{2}$.

This subject can be illustrated without difficulty by experiment. A radiant point is obtained by admitting sunlight into a darkened room through a lens of short focus placed in the window-shutter. A real image of the radiant is received upon a piece of ground glass and examined from behind. To render the light approximately homogeneous, a piece of red glass is employed. The following results relate to an equiconvex lens of 6 inches aperture and about 3 feet focus, on which the light falls obliquely.

As the screen is moved gradually back from the lens, the illuminated area diminishes. At a certain point it begins to double back upon itself, until at the primary focus the whole area is double. The light is seen to be very unequally distributed. At the edges corresponding to the boundary of the lens the illumination is feeble, while at the folded edge, corresponding to the central vertical line of the lens, a caustic is formed. On this account it would seem that curvature of the primary focal line is a worse fault than thickness for the purposes of the spectroscope.

The accompanying figures show the general character of the image at the primary focus under various circumstances. The thick line represents the folded and highly illuminated edge, the thin line the double edge corresponding to the margin of the lens. The quantities u , v_1 , v_2 are the distances

* I have lately found that the aberration of unsymmetrical pencils was very generally treated by Sir W. Hamilton in his work on Systems of Rays. Even if I could have supposed Hamilton's results to be known to the reader, the investigation in the text would still be necessary, as my purpose is very different from his. In the general theory (with β and δ finite) there is no distinction between the primary and secondary images.

from the lens of the radiant point, the primary, and the secondary focus respectively, expressed in inches.

Fig. 12.

$$\begin{aligned} u &= 39 \\ v_1 &= 28 \\ v_2 &= 94 \end{aligned}$$



Fig. 13.

$$\begin{aligned} u &= 40 \\ v_1 &= 40 \\ v_2 &= 111 \end{aligned}$$



Fig. 14.

$$\begin{aligned} u &= 40 \\ v_1 &= 60 \\ v_2 &= \text{large} \end{aligned}$$



Fig. 15.

$$\begin{aligned} u &= 40 \\ v_1 &= 117 \\ v_2 &= 168 \end{aligned}$$



Fig. 16.

$$\begin{aligned} u &= 180 \\ v_1 &= 32 \\ v_2 &= 38 \end{aligned}$$



In all these cases the line of intersection of the plane of the lens with the screen lies to the *right* of the diagram. It will be seen that the primary focal line is thin, though curved, when $v_1 = u$. This is true in general for an equiconvex lens, as may be shown from considerations of symmetry.

Experimenting on a plano-convex lens held at an obliquity of about 30° , I found that the focal lines were far better formed when the convex side was turned towards parallel rays than when the flat side was so turned. The theory, which I subsequently investigated, is given in the following section. I think that spherical lenses inclined at the most suitable obliquity might in many cases, perhaps in star-spectroscopes, replace cylindrical lenses. If it were necessary to cause no convergency at all in the secondary plane, a compensating concave lens, held perpendicularly, would be used.

[To be continued.]

LVII. *Researches on the Elementary Law of Hydrodiffusion.*
By H. F. WEBER*.

AN hypothesis respecting the elementary law according to which hydrodiffusion proceeds was first advanced by A. Fick† twenty-three years since. The great analogy which exists between the process of hydrodiffusion and that of the conduction of heat in rigid substances induced Fick to assume that the elementary law followed by hydrodiffusion is of the same form as that advanced by Fourier at the commencement of this century for the conduction of heat in rigid substances. According to that assumption, the quantity of salt which, in hydrodiffusion, flows in the direction of diminishing concentration through any element of surface in a certain element of time would be proportional to the magnitude of the surface-element considered, the length of the time-element, the value of the negative differential quotient of the concentration at the place of the surface-element in the direction of the current, and, finally, proportional to a constant the value of which depends on the nature of the salt-solution in which the hydrodiffusion takes place. This constant, which may be denoted by k , was called by Fick the “diffusion-constant.”

From this elementary law it follows that the diffusion-constant, k , signifies the amount of salt which would pass through the unit of surface during the unit of time if the decrement of concentration at the place of this unit of surface had invariably the value 1; and it follows, further, that the course of the diffusion, in a vessel in which the concentration z at any time t is only a function of a single space-coordinate x , is determined by the partial differential equation

$$\frac{\partial z}{\partial t} = k \frac{\partial^2 z}{\partial x^2}.$$

Fick, in a series of measurements, sought to test the correctness of this hypothetical elementary law by experience. He believed he might conclude from these measurements that hydrodiffusion in fact proceeds according to the defined elementary law; and he tried to fix the numerical value of the diffusion-constant for aqueous solutions of common salt. Objections have since been repeatedly urged against the method of measurement employed by Fick,—and justly, since his pro-

* Translated from a separate impression, communicated by the Author, of the memoir communicated to the Zurich Naturforschende Gesellschaft, Nov. 25, 1878.

† Pogg. *Ann.* vol. xciv.

cedure was such that it could not possibly serve for *rigorously* testing the accuracy of the assumed elementary law. Several times in the last twenty years attempts have been made to employ more exact methods of measuring, and especially those founded on optical principles, for tracing the course of hydro-diffusion,—thus by Voit* in 1866, and by Johannisjanz† in 1877. The success, however, of these supposed more delicate optical methods was no greater than that gained by Fick; and these new investigations do not at all make it evident that the elementary law assumed as an hypothesis by Fick is in all strictness the natural law that really governs what takes place in diffusion. The more recent of these investigations press upon us the conviction that either all the methods of measuring hitherto made use of for the examination of diffusion are still too rough and imperfect, or diffusion goes on according to an elementary law very different from Fick's. Thus, for example, the values calculated by Johannisjanz for the diffusion-constant of a common-salt solution, on the basis of Fick's law, from the observations of one day each, showed differences amounting to 20 per cent. from one day to another of the series of observations lasting several days; further, he found a mean value for the constant of diffusion of solution of common salt deviating about 45 per cent. from the value previously found by Fick; and Johannisjanz could perceive no influence of temperature upon the progress of the diffusion, while Fick proved a very marked influence of temperature.

In the following memoir a new method will be described which affords an extremely sharp test of the accuracy of the elementary law of hydrodiffusion, while it is of a very simple form, and permits the progress of the diffusion to be clearly ascertained almost from moment to moment.

During the last two years I have been constantly occupied with the following problem:—to discover a unit of electromotive force that could be identically reproduced at any time, and to determine its quantity in absolute measure by two methods as widely different as possible. On the occasion of those researches I was obliged, in August and September of the year 1877, to investigate thoroughly the influence of the concentration of aqueous solutions of a zinc- and a copper-salt upon the electromotive force of the galvanic elements constructed out of them. As had already previously been found, the result obtained was, *inter alia*, that the electromotive force of the Daniell element diminishes as the concentration of the sulphate-of-zinc solution bathing the zinc-electrode increases, while it increases as the degree of concentration of the copper-sulphate

* Pogg. *Ann.* vol. cxxx.

† Wiedem. *Ann.* vol. ii.

solution encompassing the copper electrode rises. It thence follows that galvanic elements which are formed of two unequally concentrated solutions of copper or zinc sulphate and two copper or zinc electrodes dipping in these solutions, must show an electromotive force of such a direction that the current produced by it in a closed circuit tends, in consequence of the migration of the ions, to equalize the existing differences of concentration. Observations by Wild and by Lindig, many years since, proved that these electromotive forces really exist, and that the currents springing from them do flow through the element in the direction of increasing concentration. The most recent experiments, made by J. Moser, have completely confirmed the earlier observations. I have repeatedly made extended series of observations for the purpose of ascertaining empirically the connexion between the intensities of these electromotive forces and the concentrations of the salt-solutions generating them. They showed that with greater concentrations the electromotive force between two electrodes of zinc, or two of copper, which are immersed in two solutions of zinc or copper sulphate of different degrees of concentration, is, within a moderate difference of concentration between the two solutions, very approximately proportional to that difference, and that for a very great difference between the concentrations it can be expressed with perfect precision by a parabolic function of the concentrations of the two liquids. In the publication of my investigations on "an empirical unit of electromotive force, and its absolute value," I will enter more fully into this subject; I here content myself with supporting the correctness of the enunciated proposition by adducing a single series of experiments.

Four solutions of sulphate of zinc were prepared, containing the following quantities (expressed in grams) of anhydrous salt in the chemical unit of volume—*i. e.* possessing the following concentrations:—

Solution I.	0.1676
Solution II.	0.2301
Solution III.	0.2858
Solution IV.	0.3213

A parallelepipedal glass trough was divided into two compartments by cementing into it a porous earthen diaphragm. By pouring into these compartments any two of the four solutions, and putting in two amalgamated zinc electrodes, a galvanic element was produced, the electromotive force of which was measured by Dubois-Reymond's modification of the compensation method. The compensating element employed

was a very constant Daniell possessing the electromotive force 11.02×10^{10} [mgr. $\frac{1}{4}$ mm. $\frac{3}{2}$ sec. $^{-2}$] and the resistance 0.98 mercury unit. The measuring-wire was a homogeneous one of German silver 1 metre in length and with a resistance of 2.22 mercury units. In the circuit which included the measuring-wire and the Daniell's element, there was moreover a rheostat resistance amounting to 110 m. u.; so that the total resistance of this circuit was 113.20 m. u. If λ denote the length of the measuring-wire that must be inserted in order to effect complete compensation, by the Daniell's element, of the electromotive force E of the combination employed, then, under the circumstances above described, the following equation holds good:—

$$E = \lambda \times 216.1 \times 10^4.$$

The following values of λ were found:—For the combination of solutions

	λ .
I. and II.	$\left\{ \begin{array}{l} 305.4 \\ 305.0 \end{array} \right\} 305.2$
I. and III.	$\left\{ \begin{array}{l} 595.5 \\ 594.3 \end{array} \right\} 594.9$
I. and IV.	$\left\{ \begin{array}{l} 795.0 \\ 794.0 \end{array} \right\} 794.5$
II. and III.	$\left\{ \begin{array}{l} 289.0 \\ 288.0 \end{array} \right\} 288.5$
II. and IV.	$\left\{ \begin{array}{l} 488.9 \\ 490.1 \end{array} \right\} 489.5$
III. and IV.	$\left\{ \begin{array}{l} 195.1 \\ 194.0 \end{array} \right\} 194.5$

These observed values can be quite satisfactorily expressed, through the concentrations z_2 and z_1 , which appear at the cathode and anode respectively of the combination employed, by the parabolic formula

$$\lambda = a[z_2 - z_1][1 + b(z_2 + z_1)].$$

If, for example, we select the combination of solutions I. and III. and that of solutions III. and IV., and calculate from the values of λ found for these combinations, and from the respective concentrations, the constants a and b , we get

$$a = 371.57,$$

$$b = 0.782.$$

From these we get, for the remaining four combinations, the values of λ :—

$$304.5, \quad 789.3, \quad 290.5, \quad 485.0,$$

which differ from the observed values by only

$$-0.7, \quad -5.2, \quad +2.0, \quad -4.5.$$

The difference between observation and calculation therefore does not in any case reach 1 per cent. of the quantity measured.

Two adjacent solutions of sulphate of zinc, with the concentrations z_2 and z_1 generate therefore, between two zinc electrodes immersed in them, an electromotive force the quantity E of which can be represented by the expression

$$E = A(z_2 - z_1)[1 + B(z_2 + z_1)] \quad . \quad . \quad . \quad (1)$$

as soon as z_2 and z_1 come within the interval from $z = 0.15$ to $z = 0.35$.

By aid of the relation (1) it becomes possible in certain cases, from the measured electromotive force which makes its appearance between two zinc electrodes immersed in solutions of zinc sulphate of different degrees of concentration, to make a sure induction respecting the amount of difference of concentration of the solutions.

On this is founded the principle of the method of measurement which I have applied to the investigation of the elementary law of hydrodiffusion.

By the reduction of the measurement of the concentration to the measurement of an electromotive force advantages are gained which are offered by none of the methods hitherto used for the investigation of diffusion: the accuracy of the measurement can be carried to almost any degree desired; the concentrations measured are not, as in previous methods, mean concentrations of thin *layers* of liquid, but are those which occur in certain *surfaces*, namely the boundary surfaces in contact with the electrodes; lastly, the method employed permits (and this might be an important advantage) the diffusion-vessel to be carried to any distance we please from the measuring-place—for instance a place free from agitation, and which offers the most constant temperature possible.

Two essentially different forms of investigation were made use of: one of them requires, like all former methods, a proportionally long time of observation, a space of several days, and, in consequence of this, prepares for the exact experimental investigation of the process of diffusion sundry difficulties and casualties; the other permits the investigation to be brought to a conclusion in a few hours, and thereby affords the advantage that one is able to decide in a simple way, and in a very brief time, some important questions, such as, for example, the influence of the concentration upon the “constant of diffusion,” or the influence of temperature upon the progress of diffusion.

First Method of Investigation.

Description.—A plane circular amalgamated zinc plate was fitted to the bottom of a glass cylinder of about 11 centims. width. Upon the zinc plate a considerably concentrated solution of sulphate of zinc, freed from air, was poured, up to the height of l_2 centims. The concentration varied in each series of experiments between 0.25 and 0.35. A thin disk of cork was then laid upon the free surface of the solution, upon which a much less concentrated solution (the concentration chosen between 0.15 and 0.20) was let trickle slowly from a finely drawn-out glass tubule. This latter solution slowly spread over the lower more concentrated one; and a dividing surface formed between the two solutions, which was perfectly plane (except only along the margin, where a capillary action was visible in a zone of about 1.5 millim. breadth) and reflecting. When the second solution had attained the thickness l_1 centims. the supply was interrupted, and a plane circular amalgamated zinc plate, exactly fitting into the glass cylinder, was cautiously let down by means of a guide until complete contact took place between the upper bounding surface of the upper layer of salt and the lower surface of the zinc plate. Herewith the experiment was ready for the measurements, which consisted in measuring, at certain, usually equidistant moments of time, the electromotive force present between the two zinc plates of the diffusion-vessel. By determining the time-rate of this electromotive force, we gain, as the following theoretic treatment will show, a means for a very delicate testing of the accuracy of Fick's elementary law of hydrodiffusion.

Theory.—We have first, from Fick's elementary law and the realized conditions of the experiment, to ascertain that function which represents the value of the concentration z , of any layer at the depth x below the upper electrode, at any time t .

The concentration z has at all times, and at every place between $x=0$ and $=l_1+l_2$, to fulfil Fick's elementary law—that is, to satisfy the partial differential equation

$$\frac{\partial z}{\partial t} = k \frac{\partial^2 z}{\partial x^2}, \quad \dots \quad (1)$$

The limiting conditions of the experiment are—

For $x=0$, for all values of t ,

$$\frac{\partial z}{\partial x} = 0, \quad \dots \quad (2)$$

For $x=l_1+l_2=L$, for all values of t ,

$$\frac{\partial z}{\partial x}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

since at the upper electrode no salt goes out, and at the lower electrode no salt enters. If the moment of placing in layers one over the other the two concentrations z_2 and z_1 be taken for the initial point of time, the following hold as the initial conditions of the experiment:—For $t=0$,

$$\left. \begin{aligned} z &= z_1 \text{ for all values of } x \text{ from } x=0 \text{ to } x=l_1; \\ z &= z_2 \quad , , \quad , , \quad , , \quad x=l_1 \text{ to } x=l_1+l_2=L. \end{aligned} \right\} \quad (4)$$

A particular integral of the differential equation (1) is

$$z=(A \cos hx + B \sin hx)e^{-h^2kt}.$$

The three constants A , B , and h are to be determined from the initial and limiting conditions of the experiment.

In the first place,

$$\frac{\partial z}{\partial x}=(-hA \sin hx + hB \cos hx)e^{-h^2kt}$$

in the layer $x=0$ is to have the value 0 for all values of t ; hence B must be put $=0$. Further, the same differential quotient for $x=L$ is likewise to vanish for all values of t ; the constant h must therefore be so chosen as to satisfy the equation

$$hL=n\pi,$$

where $n=0, 1, 2, 3 \dots$

The sum of the particular integral

$$z=n \sum_0^{\infty} A_n \cos \left(\frac{n\pi}{L} x \right) e^{-\frac{n^2\pi^2}{L^2} kt}$$

gives the general integral. The constant A , yet to be determined, can be ascertained from the initial conditions (4):—For $t=0$,

$$z=n \sum_0^{\infty} A_n \cos \left(\frac{n\pi}{L} x \right) = z_1 \text{ for all values of } x \text{ from } x=0 \text{ to } x=l_1;$$

and

$$z= \sum_0^{\infty} A_n \cos \left(\frac{n\pi}{L} x \right) = z_2 \text{ for all values of } x \text{ from } x=l_1 \text{ to } x=l_1+l_2=L.$$

According to Fourier's theorem we have

$$A_0=\frac{1}{L} \int_0^L (z)_{t=0} dx = \frac{z_1 l_1 + z_2 l_2}{L}$$

and

$$A_n = \frac{2}{L} \int_0^L (z)_{t=0} \cos\left(\frac{n\pi x}{L}\right) dx = -\frac{2(z_2 - z_1)}{\pi} \frac{1}{n} \sin\left(\frac{n\pi l_1}{L}\right).$$

The general solution, taking into account all the conditions, is therefore:—

$$z = \frac{z_1 l_1 + z_2 l_2}{L} - \frac{2(z_2 - z_1)}{\pi} \sum_1^{\infty} \frac{1}{n} \sin\left(\frac{n\pi l_1}{L}\right) \cos\left(\frac{n\pi x}{L}\right) e^{-\frac{n^2 \pi^2}{L^2} kt}. \quad (5)$$

The above-described method of experiment permits only the variable concentrations of the *boundary layers* (for $x=0$ and $x=L$) to be observed. The variable concentration of the upper boundary layer may be denoted by z' , that of the lower by z'' . From (5) we find

$$z' = \frac{z_1 l_1 + z_2 l_2}{L} - \frac{2(z_2 - z_1)}{\pi} \left\{ \sin\left(\frac{\pi l_1}{L}\right) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{2} \sin\left(\frac{2\pi l_1}{L}\right) e^{-\frac{4\pi^2}{L^2} kt} + \frac{1}{3} \sin\left(\frac{3\pi l_1}{L}\right) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\},$$

and

$$z'' = \frac{z_1 l_1 + z_2 l_2}{L} + \frac{2(z_2 - z_1)}{\pi} \left\{ \sin\left(\frac{\pi l_1}{L}\right) e^{-\frac{\pi^2}{L^2} kt} - \frac{1}{2} \sin\left(\frac{2\pi l_1}{L}\right) e^{-\frac{4\pi^2}{L^2} kt} + \frac{1}{3} \sin\left(\frac{3\pi l_1}{L}\right) e^{-\frac{9\pi^2}{L^2} kt} - \dots \right\};$$

therefore

$$z'' - z' = \frac{4(z_2 - z_1)}{\pi} \left\{ \sin\left(\frac{\pi l_1}{L}\right) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{3} \sin\left(\frac{3\pi l_1}{L}\right) e^{-\frac{9\pi^2}{L^2} kt} + \frac{1}{5} \sin\left(\frac{5\pi l_1}{L}\right) e^{-\frac{25\pi^2}{L^2} kt} + \dots \right\},$$

and

$$z'' + z' = 2 \frac{z_1 l_1 + z_2 l_2}{L} - \frac{4(z_2 - z_1)}{\pi} \left\{ \frac{1}{2} \sin\left(\frac{2\pi l_1}{L}\right) e^{-\frac{4\pi^2}{L^2} kt} + \frac{1}{4} \sin\left(\frac{4\pi l_1}{L}\right) e^{-\frac{16\pi^2}{L^2} kt} + \dots \right\}.$$

In all the experiments made, L was constantly chosen so small (about 3 centims.) that the values of the terms

$$\frac{1}{4} \sin\left(\frac{4\pi l_1}{L}\right) e^{-\frac{16\pi^2}{L^2} kt}$$

and

$$\frac{1}{5} \sin\left(\frac{5\pi l_1}{L}\right) e^{-\frac{25\pi^2}{L^2} kt} \dots$$

already, after the lapse of one day ($t=1$), had sunk below $\frac{1}{400}$. Moreover L_1 was always made as nearly as possible $=\frac{L}{3}$, in order to bring the term $\frac{1}{3} \sin\left(\frac{3\pi l_1}{L}\right) e^{-\frac{9\pi^2 kt}{L^2}}$ to vanishing. In all the experiments carried out, therefore, for every instant of time $t > 1$, was

$$z'' - z' = \frac{4(z_2 - z_1)}{\pi} \sin\left(\frac{\pi}{3}\right) e^{-\frac{\pi^2}{L^2} kt},$$

and

$$z'' + z' = 2 \frac{z_1 l_1 + z_2 l_2}{L} - \frac{2(z_2 - z_1)}{\pi} \sin\left(\frac{2\pi}{3}\right) e^{-\frac{4\pi^2}{L^2} kt}.$$

From this results, as the expression of the electromotive force E generated by the concentrations z'' and z' of the boundary layers between the two electrodes at the moment $t (> 1)$,

$$\begin{aligned} E &= A(z'' - z'([1 + B(z'' + z')]) \\ &= A \frac{4(z_2 - z_1)}{\pi} \sin\left(\frac{\pi}{3}\right) e^{-\frac{\pi^2}{L^2} kt} \left\{ 1 + 2B \left(\frac{z_1 l_1 + z_2 l_2}{L} \right. \right. \\ &\quad \left. \left. - \frac{(z_2 - z_1)}{\pi} \sin\left(\frac{2\pi}{3}\right) e^{-\frac{4\pi^2}{L^2} kt} \right) \right\}. \end{aligned}$$

If, therefore, Fick's law of hydrodiffusion is correct, then the electromotive force between the two electrodes must be such a function of the time that

$$E = A_1 e^{-\frac{\pi^2}{L^2} kt} - B_1 e^{-\frac{5\pi^2}{L^2} kt},$$

where A_1 and B_1 are brief designations of certain constant values.

Since, according to the above-given measurements, the value of the constant B is very small compared with the value of the constant A , the expression of the electromotive force will after a short time reduce to the first term, to

$$E = A_1 e^{-\frac{\pi^2}{L^2} kt}.$$

The measuring of the electromotive force was accomplished according to Dubois-Reymond's modification of the compensation method. A Daniell element served as compensator, of which the electromotive force never varied more than $\frac{1}{1300}$ of its value. The measuring-wire employed was perfectly equal in value in each of its parts; moreover care was taken that the resistance of the Daniell and the other resistance of the galvanic circuit in which the compensating Daniell was included remained the same during all the measurements. If

λ denotes that length of measuring-wire which must be inserted as the part common to both galvanic circuits in order to produce in the second circuit perfect compensation of the electromotive force to be measured, w the resistance of unit length of the measuring wire, W the total resistance of the first circuit, and D the electromotive force of the compensating Daniell, the expression of the electromotive force to be measured is

$$E = \frac{\lambda \cdot w}{W} \cdot D.$$

The length λ of the measuring-wire which must be inserted at the time t in order to fully compensate by the Daniell's element the electromotive force present at that time between the two zinc electrodes is accordingly connected with the time in such wise that

$$\lambda = \frac{W}{wD} \left[A_1 e^{-\frac{\pi^2}{L^2} kt} - B e^{-\frac{5\pi^2}{L^2} kt} \right], \text{ for } t > 1,$$

and, for greater values of t ,

$$\lambda = \frac{W}{wD} A_1 e^{-\frac{\pi^2}{L^2} kt}.$$

Hence it follows that, if for a series of equidistant moments of time $t_0, t_1, t'_2 \dots$, at the production of compensation of the electromotive forces corresponding to the concentrations of the boundary layers, the lengths of measuring-wire inserted have been found equal to $\lambda_0, \lambda_1, \lambda_2 \dots$, then must the quantity $lg\left(\frac{\lambda_i}{\lambda_{i+1}}\right)$ very slightly rise with increasing time, and consequently with increasing i , and pretty soon become constant. The constant value which this quantity finally reaches is

$$lg\left(\frac{\lambda_i}{\lambda_{i+1}}\right) = \frac{\pi^2}{L^2} k \Delta t,$$

in which $\Delta t = t_1 - t_0 = t_2 - t_1 = t_3 - t_2$, &c. With the aid of this constant value (call it η) the quantity of Fick's "constant of diffusion" can be ascertained; we get

$$k = \eta \frac{L^2}{\pi^2} \frac{1}{\Delta t}.$$

Results of the Observations.—The first two series of observations were made in an apartment at the Physical Institute in the course of October 1878. In consequence of the warming,

the temperature of the room regularly rose during the forenoon, till about twelve o'clock, about 2° C., to fall exactly as much, extremely slowly and regularly, from that time till the next morning. The observations, which in each series were carried on throughout both day and night, at intervals of three hours each, showed that the diffusion proceeded with the utmost regularity during the period from about 1 P.M. to about 7 A.M.; while for the six hours of the forenoon they gave, day for day, a somewhat devious course. This indicated that variations of temperature even to the amount of only about 2° considerably disturb the course of diffusion, and that pure results can only be obtained at as nearly as possible constant temperature of the diffusion-vessel. (From the experiments afterwards communicated it will be seen that the method of observation I have employed is so sensitive that it makes most distinctly evident the influence of a temperature-variation of as little as $\frac{1}{5}^{\circ}$.) Professor Wolf had the kindness to place at my disposal for a long time the cellar-rooms of the Federal Observatory. I there carried out two very protracted series of observations. Unfortunately, in the middle of the first series a sudden and very considerable fall of the outside temperature occurred, the influence of which, although much weakened, extended down even into the cellars. Hence I forbear to communicate the results of that series of observations. During the second series the temperature varied extremely little and very slowly, as may be seen from the readings given in the following Table (p. 499); for this second series the experiment was arranged as follows.

On the 9th of November a solution of zinc sulphate, freed as much as possible from air, and possessing the concentration $z_2 = 0.3182$ (that is, a cubic centim. of solution contained 0.3182 gram of anhydrous salt), was brought upon the lower amalgamated zinc electrode up to the height $l_2 = 1.98$ centim. At 7 A.M. to the moment, over this solution a second, with the lower concentration $z_1 = 0.1520$, was superposed, to the height $l_1 = 1.01$ centim., so that the surface of separation between the two solutions proved perfectly even and reflecting. Then the second zinc electrode was lowered, by means of a suitable guide, upon the upper bounding surface of this second layer till it came into contact with the solution, and was fixed in that position. The diffusion-vessel having been closed airtight by means of paper steeped in bees'-wax and some additional bees'-wax, it remained untouched upon a pillar of the room during the whole twelve days of the observation. The first reading was made immediately after the preparation of the experiment; the further readings followed six times daily,

at the close of every three hours ; so that in this series of observations 72 data were obtained.

As was mentioned above, the compensation method was used for measuring the electromotive forces. The compensating Daniell element possessed an almost perfectly constant electromotive force : the ascertained variations of it never exceeded $\frac{1}{1300}$ of its mean value. Besides the Daniell element there was another resistance in the first circuit, of 130 mercury units. The resistance of the measuring wire, which was 1 metre in length and perfectly homogeneous, amounted to 2.221 mercury units. The galvanometer of the second circuit was rendered so sensitive that the rectification could be made upon the measuring-wire exactly to 0.1 millim. Immediately after the preparation of the experiment, λ was found to be =976.1 millims. ; twelve hours later it had already fallen to 951.6. During the next twelve hours it diminished 80.1 millims., and in twelve hours more almost the same amount. The high degree of delicacy and exactness of the method employed can be perceived from these numbers. A change in the difference of the concentrations of the two boundary layers equal to the 800th part of that alteration of the concentration-difference which took place during half a day could be perceived with absolute certainty ; the value of λ diminished during the first days from minute to minute about 0.1 millim. ; the progress of the diffusion could thus be as it were tangibly traced from minute to minute. The initial difference of the concentrations of the boundary layers, $z_2 - z_1 = 0.1562$, corresponded to $\lambda = 976.1$ millims. On account of the very close proportionality between the concentration-difference and the electromotive force, an alteration of the concentration-difference amounting to 0.016 milligram corresponds to an alteration of the value of $\lambda = 0.1$ millim. *Therefore, if the lower boundary layer lost by diffusion only $\frac{1}{120}$ milligram of salt per cubic centimetre, and at the same time the upper boundary layer gained the same amount, this effect of diffusion could still be distinctly perceived.*

The following Table gives all the particulars of the fourth series of observations. In the first column stands the date ; the second contains the temperature of the apartment at the time of each observation ; the third gives the rectifications λ upon the measuring-wire ; the last column contains, from the second day onwards, the differences of the ordinary logarithms of each two successive values of λ .

	T=	λ=	Δ log=		T=	λ=	Δ log=
	°	mm.			°	mm.	
Nov. 9.	9·8	976·1		Nov. 15.	9·5	320·4	0·01126
	9·8	974·8			9·4	312·2	0·01141
	9·8	969·4			9·5	304·1	0·01114
	9·9	961·0			9·6	296·4	0·01098
	9·9	951·6			9·6	289·0	0·01127
	9·8	939·0			9·5	281·6	
Nov. 10.	9·6	871·5	0·01034	Nov. 16.	9·4	260·1	0·01116
	9·7	849·9	0·01075		9·6	253·5	0·01093
	9·7	830·1	0·01022		9·5	247·2	0·01085
	9·8	810·8	0·01046		9·5	241·1	0·00984
	9·8	791·5	0·01060		9·6	235·7	0·01120
	9·7	772·5			9·4	229·7	
Nov. 11.	9·5	717·8	0·01028	Nov. 17.	9·4	212·5	0·01076
	9·6	701·0	0·01079		9·6	207·3	0·01125
	9·6	683·8	0·01061		9·7	202·0	0·01110
	9·8	667·3	0·01074		9·6	196·9	0·01095
	9·7	651·0	0·01060		9·6	192·0	0·01123
	9·7	635·3			9·7	187·1	
Nov. 12.	9·6	589·0	0·01053	Nov. 18.	9·5	173·2	0·01041
	9·7	574·9	0·01063		9·5	169·1	0·01092
	9·9	561·0	0·01058		9·4	164·9	0·01093
	9·8	547·5	0·01149		9·6	160·8	0·01084
	9·8	533·2	0·01047		9·5	156·8	0·01066
	9·7	520·5			9·4	153·0	
Nov. 13.	9·5	483·2	0·01102	Nov. 19.	9·5	141·2	0·01058
	9·7	471·1	0·01158		9·5	137·8	0·01117
	9·7	458·7	0·01103		9·6	134·3	0·01081
	9·6	447·2	0·01131		9·6	131·0	0·01176
	9·8	435·7	0·01101		9·5	127·5	0·01069
	9·6	424·8			9·5	124·4	
Nov. 14.	9·4	394·0	0·01106	Nov. 20.	9·4	115·0	0·01109
	9·5	384·1	0·01168		9·5	112·1	0·01099
	9·5	373·9	0·01047		9·6	109·3	0·01046
	9·5	365·0	0·01084		9·5	106·7	0·01154
	9·6	356·0	0·01050		9·4	103·9	0·01058
	9·5	347·5			9·4	101·4	

These numbers show, at a glance, that the electromotive force between the two zinc electrodes, exactly in accordance with the demands of the theory, sank to smaller and smaller values as the time increased, according to the law

$$E = A_1 e^{-\frac{\pi^2}{L^2} kt} - B_1 e^{-\frac{5\pi^2}{L^2} kt}.$$

The smallness of the coefficient B_1 in proportion to A_1 , and the shortness of the distance L between the electrodes, had for their effect that already from the fourth day the second term in the preceding expression furnished to the value of E only a vanishingly small amount, and the electromotive force from

that time forth varied, with increasing time, in the form of a simple geometrical progression,

$$E = A_1 e^{-\frac{\pi^2}{L^2} kt}.$$

If we calculate by aid of this latter relation the value of the constant $\eta = \frac{\pi^2}{L^2} k$ from all the observations of each two consecutive days according to the method of least squares, we find from the observations of

				Mean temperature.
13th & 14th Nov.	$\eta = 0.2032$	.	.	9.6
14th „ 15th „	$\eta = 0.2066$	.	.	9.5
15th „ 16th „	$\eta = 0.2045$	.	.	9.5
16th „ 17th „	$\eta = 0.2027$	.	.	9.5
17th „ 18th „	$\eta = 0.2027$	.	.	9.5
18th „ 19th „	$\eta = 0.2049$	.	.	9.5
19th „ 20th „	$\eta = 0.2049$	.	.	9.4

The mean values from the first four, the last four, and from all these values are:—

$$\eta = 0.2042$$

$$\eta = 0.2038$$

$$\eta = 0.2042$$

Certainly these numbers scarcely permit a doubt to arise about this, that the quantity which according to the theory should be constant was in fact constant for the experiment-conditions here realized. Fick's elementary law is, accordingly, for those differences of concentration which were present in the diffusion-vessel from the fourth day, indeed the exact expression of the progress of the diffusion.

From the above mean value for $\eta = \frac{\pi^2}{L^2} k$, and the value of L (2.99 centims.), the following is found as the value of Fick's diffusion-constant:—

$$k = 0.1849 \left[\frac{\text{centim.}^2}{\text{day}^1} \right], \text{ valid for the mean temperature } 9^{\circ}5.$$

[To be continued.]

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE SPECIFIC HEATS AND MELTING-POINTS OF DIVERS

REFRACTORY METALS. BY J. VIOLLE.

I. **T**HE specific heat of iridium, like that of platinum, increases regularly with the temperature. The experiments were extended up to 1400° ; and they are well represented by the formula found for platinum,

$$C_0 = 0.0317 + 0.000006t.$$

From this the following values are deduced for the mean specific heat between zero and t degrees:—

C_0^{100}	0.0323	C_0^{800}	0.0365
C_0^{200}	0.0329	C_0^{900}	0.0371
C_0^{300}	0.0335	C_0^{1000}	0.0377
C_0^{400}	0.0341	C_0^{1100}	0.0383
C_0^{500}	0.0347	C_0^{1200}	0.0389
C_0^{600}	0.0353	C_0^{1300}	0.0395
C_0^{700}	0.0359	C_0^{1400}	0.0401

It was not without some little difficulty that the melting-point of iridium could be determined; that metal in fact necessitates the employment of hydrogen and oxygen pure and dry in the blowpipe of Deville and Debray; and to melt 20 grams of iridium not less than 500 litres of hydrogen and 250 litres of oxygen must be consumed. Nevertheless three experiments were brought to a successful issue, conducted in the manner indicated for platinum, and in which 24, 8.970, and 8.404 grams of solid iridium gave up to the calorimeter, at the temperature of the fusion, respectively 84.2, 85.3, and 83.9 units of heat per gram of metal, or a mean of 84.5 units. If, then, it be admitted that the formula above given represents the specific heat of iridium up to its melting-point, a temperature certainly very little below that of the blowpipe-flame, it follows that iridium fuses at 1950° C.

II. Gold presents a mean specific heat which up to 600° hardly varies at all, then sensibly increases in proportion as the melting-point is approached: equal to 0.0324* (according to Regnault) between 0° and 100° , still nearly the same at 600° , it reaches 0.0345 at 900° and 0.0352 at 1020° . The melting-point of gold, determined as usual, is 1035° .

III. The melting-point of copper is very close to that of gold, but a little higher†: pure copper melts at 1054° .

IV. Collecting in a Table the melting-points given in this and

* Gold of $\frac{999}{1000}$. I found a specific heat a little less, $C_0^{100} = 0.0316$, with a specimen of perfectly pure gold which I used in my researches, and for which also I am indebted to the kindness of M. Debray.

† The red copper of commerce melts before virgin gold, 15° to 30° before gold like the specimen.

previous Notes, we have the following numbers, all referring to the air-thermometer :—

Silver	954°	Palladium.....	1500°
Gold.....	1035	Platinum.....	1775
Copper.....	1054	Iridium	1950

—*Comptes Rendus de l'Académie des Sciences*, t. lxxxix. No. 17, pp. 702, 703.

RESEARCHES ON DALTONISM. NOTE BY J. MACÉ AND W. NICATI.

I. Thanks to the kindness of the Principals of the Lyceums of Marseilles and Grenoble, and of the Principal of the College of Aix in Provence, we have been able to examine a total of 925 boys, of whom 33 were daltonians, or 3·57 per cent. We have examined a smaller number of girls, or 241, of whom one only was daltonian. For these elementary researches we have employed Seebeck's method so ingeniously improved by M. Holmgren, consisting in getting the examinees to select, from a mass of specimens of coloured wool, those which are like some suitably chosen types, without troubling about the names, more or less accurate, that may be given to the colours. These researches have extended to too small a number of individuals to possess any real statistical value; nor was that our aim. They are nevertheless to a certain degree interesting from their agreement with the results obtained by Holmgren, Jeffries, and others abroad. On the other hand, they are utterly different from the results obtained by Dr. Favre, who finds from 20 to 30 daltonians per hundred individuals examined.

II. The principal object of our investigations has been to obtain comparative measures between the quantities of light perceived in the different parts of the spectrum by the daltonian and by the normal eye. In the experiments which we relate today we employed an indirect method. It is based upon the fact that the acuteness of vision diminishes simultaneously with the intensity of the light; and it consists in measuring the visual acuteness of the daltonian in the different portions of the spectrum, always comparing it with the visual acuteness, in the same circumstances, of a normal sight. We make use of a spectrum with a total length of more than 5 decimetres, thrown upon a stretched graduated rule of black velvet. Over this rule slides at will a square of white cardboard, from which a letter of the alphabet is cut out 5 millims. in diameter. (Letters written with ordinary ink fluoresce very inconveniently in the violet.) The measuring consists in ascertaining the maximum distance d at which the daltonian must place himself in order to distinguish the character, and immediately afterwards the corresponding distance D for one of us, always the same individual.

Determining in this way the values of the ratio $\frac{d}{D}$ for the various regions of the spectrum, we have obtained curves which may be referred to three types: one set, three in number, descend towards the red end of the spectrum; another curve descends towards the

violet end ; and two present a minimum of perception in the green. (We here give only the observations made upon eyes of absolutely normal refraction and visual acuteness, and consequently giving the surest guarantees of correctness.) We must particularly insist upon the instances of descent of the curve in the green, which we believe we are among the first to verify with certainty. Here are the figures of one of the experiments for a length of 7 centims. of the spectrum taken in the interesting region :— $\frac{d}{D}=1.09, 0.83, 0.76,$

1.02. The minimum of acuteness sensibly corresponds to the middle of the distance between the lines E and F in the green. In this latter point d and D differ by 6 decimetres ; and fourteen measurements effected over the whole extent of the spectrum give a perfectly regular curve.

Let it not surprise that in the observation just reported the visual acuteness in the defective region still corresponds to about three fourths of the normal. Direct experiments have shown us that the visual acuteness varies slowly when the light diminishes in a considerable proportion. Thus it was that, in one of the experiments made on this question, the intensity of the light varying in the ratio of 1 to 4, the distance at which one of us had to place himself to recognize the character varied only in the ratio of 1 to 1.34. We have not, however, been able to discover any simple relation between visual acuteness and intensity of light ; and we are engaged in transforming the present apparatus so as to leave the visual acuteness constant while making the intensity of the light alone to vary.

III. It follows from these first observations that the observed facts are not at all in contradiction with the theory of Young and Helmholtz on the perception of colours. In fact they correspond to the three types of daltonians (for the red, for the violet, and for the green) foreseen by that theory.

We have controlled the obtained results by comparing the visual acuteness of the normal with that of the daltonian eye when both look through one and the same coloured substance. A red glass, for example, which scarcely lessens the sight of a normal eye, diminishes extraordinarily that of the daltonian for the red ; and a green glass and a blue or violet glass do the same for the other daltonians. This procedure, very delicate and not very expensive, can, up to a certain point, be substituted for the method that consists in measuring the visual acuteness by means of variously coloured alphabets.—*Comptes Rendus de l'Académie des Sciences*, Oct. 27, 1879, t. lxxxix. pp. 716–718.

ON A NEW HYGROMETER. BY FR. SCHWACKHOFER*.

First a determined volume of the air to be examined is enclosed in a burette, and thence conducted into a second vessel, in which the aqueous vapour is absorbed by concentrated sulphuric acid. The dry air is returned to the burette, and the change of volume measured. The volume-hygrometer gives very accurate results

* *Zeitschrift d. allg. österr. Apotheker-Vereins*, 1879, No. 2, p. 2.

under all circumstances, even at temperatures below zero at which the psychrometer will not serve. A determination takes fifteen minutes. After finishing one, the apparatus can, without any further preparation, be used for a second; and once filling with sulphuric acid suffices for many thousand determinations. Volume-percentages are read off directly from the apparatus. For calculating percentages of volume into millimetres vapour-pressure the formula $e = V \frac{b}{100}$ is employed, in which V denotes the volumes, e and b the heights of the barometer. The same apparatus can also be used for fog-measurement. For that purpose we warm the foggy air a little, before it enters the volume-hygrometer, and then proceed as usual: we thus obtain the total amount of the aqueous vapour, consisting of that which was present as such in the air and possessed the maximum of tension, plus that which was first formed by the volatilization of the fog vesicles.—E. W. in the *Beiblätter zu den Annalen der Physik und Chemie*, 1879, No. 7, p. 485.

ON THE GALVANIC OXIDATION OF GOLD. BY M. BERTHELOT.

Grotthuss, in his classic experiments on the decomposition of water by the galvanic pile*, remarked the solution of a gold wire employed as the positive pole in sulphuric acid traversed by the current. This interesting fact was pointed out to me by our venerated dean, M. Chevreul, who asked me if such an effect might not be due to the formation of persulphuric acid. It was for the purpose of replying to his question that I made the following experiments.

I first repeated Grotthuss's experiment, which is precisely as he described it. The sulphuric acid (at 10 per cent.) becomes yellow and rapidly dissolves the gold wire: the dissolved gold can easily be detected by means of stannous chloride. A portion is reprecipitated upon the negative pole.

Nitric acid, under the same conditions, equally attacks gold, and becomes filled with a violaceous precipitate (gold or aurous oxide?), which remains in suspension.

Dilute phosphoric acid, on the contrary, does not appreciably attack gold, even under the influence of the galvanic current; nor has potass any greater action.

That gold is attacked by sulphuric and nitric acids is not due to ozone; for oxygen charged with ozone remains without action upon gold in the presence of water, whether pure or charged with sulphuric or nitric acid.

Nor does persulphuric acid (prepared by electrolysis) attack gold, even if it contains in addition a portion of oxygenated water.

It follows from these observations that the attacking of gold takes place solely under the influence of the galvanic current and at the contact of the electrode with the electrolyzed liquid.—*Comptes Rendus de l'Académie des Sciences*, Oct. 27, 1879, p. 683.

* *Annales de Chimie*, t. lxxviii. p. 60.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

SUPPLEMENT to VOL. VIII. FIFTH SERIES.

LIX. *Action of Magnets on Mobile Conductors of Currents.*
By Prof. SILVANUS P. THOMPSON, B.A., D.Sc.*

[Plate XIV.]

IN studying the phenomena of the voltaic arc the author has been led to inquire into the actions produced by magnets upon movable portions of circuits traversed by electric currents.

It is not easy to construct a mechanism having a given number of degrees of freedom which shall at the same time possess sufficient mobility and freedom from friction to admit of the experimental study of the forces exerted upon such a system by the joint action of an external magnetic field and of a current traversing the conductor thus constructed. Many different forms of apparatus have been devised for exhibiting motions of translation and rotation produced by the attractions and repulsions due to such joint actions; but in all these the motion was necessarily limited to one or two degrees of freedom.

The principal experimental appliances which have been employed for the production of rotation or translation between a conductor carrying a current and a magnet or an electromagnet, may be grouped as follows :—

- I. Systems of jointed conductors;
- II. Systems of bendable conductors;
- III. Systems of conductors of which a liquid constitutes a part;
- IV. Systems of conductors of which the voltaic arc constitutes a part;
- V. Systems of conductors of which the luminous discharge through a rarefied medium forms a part;
- VI. Other systems—liquid veins, flames, steam-jets, &c.

* Communicated by the Author.

I. The systems of jointed conductors, dating from the apparatus of Ampère and Faraday, are of innumerable forms, and are generally so well known that there is no need further to specify them.

II. *Systems of Bendable Conductors*.—Cumming* devised the elegant experiment of passing a current through a strip of gold-leaf which hung vertically between the horizontal poles of a horse-shoe magnet. The motion of the flexible strip from or towards the magnet served to indicate the direction of the current. A telegraph on this principle was suggested by Highton, and even patented, but never brought into practical operation.

Le Roux†, in 1860, studied the action of a powerful magnet upon a long spiral coil of thin platinum wire raised to near its fusing-point by the passage of a current through it. It bent about in a singular manner under the influence of the magnet.

III. *Systems of Conductors of which a Liquid forms a Part*.—Davy‡ was the first to show the rotation of a conducting liquid under the influence of a magnet, employing mercury for that purpose. Later, Poggendorff§ employed the same substance in an important research on the electrodynamic rotation of liquids.

Liquids which possess electrolytic conductivity were shown by Schweigger||, in 1826, to be capable of exhibiting similar phenomena; and such liquids have been employed in the researches of Fechner¶, Ritchie**, Wartmann††; Jamin‡‡, De la Rive§§, Bertin||, and Planté¶¶. A summary of most of the results of these experimenters is given in Wiedemann's *Galvanismus****. One of Jamin's experiments deserves to be more generally known: if the pole of a powerful electromagnet be placed between the electrodes of a water voltmeter, it is observed that the bubbles of gas which are being evolved at the two electrodes are whirled off by curvilinear currents of liquid which rotate in opposed senses.

* 'Electrodynamics.' See also Noad's 'Manual of Electricity,' p. 657 (edition of 1859).

† *Annales de Chimie et de Physique*, [3] lxi. 1860, p. 409.

‡ Phil. Trans. 1823, p. 153. § Pogg. Ann. lxxvii. 1849, p. 1.

|| Schweigg. Journ. 1826, Bd. xlviii. ¶ Ibid. 1829, Bd. lvii.

** Phil. Trans. 1832, ii. p. 294.

†† *Ann. de Chim. et de Phys.* 1847, [3] xix. p. 394.

‡‡ *Ibid.* 1855, [3] xliii. p. 334. §§ *Ibid.* 1859, [3] lvi. p. 286.

||| *Ibid.* 1860, 1865, and 1869, [4] xvi. p. 74.

¶¶ *Bibl. Univ. de Genève*, 1860, t. vii. p. 332, and *Recherches sur l'Électricité*, pp. 156 and 171, 172.

*** *Galvanismus*, vol. ii. p. 146.

IV. *Systems of Conductors of which the Electric Arc forms a Portion.*—In 1821, Davy* observed that the electric arc was affected by a magnet, being apparently drawn towards, or repelled from between, the poles of a magnet, according to the direction of the current and the position of the poles of the magnet.

In 1844 Casselmann† conducted a research on the influence of the earth's magnetism upon the arc when placed in various positions. When the arc was horizontal, and so placed that the current through it flowed from north to south, the arc was deflected towards the eastern side; when from

south to north,	the arc was deflected towards the west ;
west to east,	" " " north ;
east to west,	" " " south.

Later, Walker‡ found that when the arc is made to pass from a pole of carbon to one of iron, the iron being at the same time powerfully magnetized, the arc rotates round the iron pole, the rotation taking effect in a sense opposed to that assumed by Ampère for the hypothetical molecular currents.

Quite recently two lamps have been constructed in which this phenomenon is turned to account. M. Fernet has in this way sought to increase the effective illumination obtainable in a given direction with a pair of carbon poles arranged as usual, one above the other in a regulator, a powerful magnet being placed behind to compel the arc to maintain a position in front of the carbon poles between which it is produced.

M. Jamin§ has constructed an electric candle in which the arc is maintained at the extreme end of the two parallel pencils of carbon by the influence of an external conductor of rectangular form traversed in a parallel direction by the same current.

V. *Systems of Conductors of which the Luminous Discharge through a Rarefied Medium forms a Part.*—Plücker|| first drew attention to the remarkable effects produced by a magnet on the luminous discharges in the Geissler tubes. De la Rive's¶ beautiful apparatus for demonstrating the actual rotation of the discharge about the pole of a powerful magnet is also too well known to require further comment. More recently Mr. Crookes** has devised a number of striking experiments, which illustrate in greater detail the laws of electrodynamics which determine the movements of currents.

* Phil. Trans. 1821, ii. p. 427.

† Pogg. Ann. 1844.

‡ Transactions of the Electrical Society, 1837-40.

§ *Journal de Physique*, 1879.

|| Pogg. Ann. ciii. 1856, p. 88 &c.

¶ Pogg. Ann. civ. p. 129.

** Proc. Roy. Soc. 1879.

These experiments have moreover demonstrated that the convection of a quantity of electricity by a flight of electrified molecules is, electromagnetically, the same thing as the conduction of the same amount of electricity by a conductor—these flights of electrified molecules, which usually take place in straight lines normally to the surface of the negative pole of the exhausted tube, taking curved paths under the influence of a magnetic field.

VI. *Systems of Conductors of which a Liquid Vein forms a Part.*—The present author has experimented on the action of magnets upon liquid veins traversed by currents, and has observed the following phenomena:—

A liquid vein traversed by a current obeys a similar law of displacement to that obeyed by a movable wire conductor under the same circumstances. A horse-shoe electromagnet was placed with its poles horizontal; and a vein of dilute sulphuric acid was made to flow vertically downwards from a reservoir above into a vessel below; and a current from a small Grove's battery was made to traverse the vein. When the circuit of the electromagnet was closed the vein was drawn aside, being attracted towards or repelled from between the poles of the electromagnet, according to the sense of the current and the direction of the magnetization, just as in the well-known apparatus of Marsh*.

When a vein of mercury was substituted for one of dilute acid, these attractions and repulsions became much more marked. It was also curious to observe that the vein, instead of falling simply obliquely, fell in a path which curved more and more away from the perpendicular (fig. 1, Plate XIV.). The author did not succeed in imparting a double curvature to the vein.

When a liquid vein carrying a current flows near the pole of a bar electromagnet whose axis is placed vertically, the vein is bent aside, the flexure being such as to tend to bring the vein into parallelism with the hypothetical Ampèrian currents (fig. 2, Plate XIV.). If the vein falls against the magnet-pole or into an annular trough surrounding the pole, a motion of reaction is set up between the vein and that against which it falls, tending to make it rotate about the magnet-pole in a sense opposed to that of the Ampèrian currents, precisely as the voltaic arc observed by Walker, and the luminous discharge in a partial vacuum by De la Rive.

When a liquid vein carrying a current falls exactly upon the pointed pole of a powerful magnet placed vertically below it, instead of being subjected to a rotatory displacement around

* Noad, *op. cit.* p. 665.

the pole of the magnet, it is subjected to a force of rotation around its own axis, which, becoming greater as it approaches the magnet-pole, produces a twisted appearance in the vein. The sense of the twist depends upon the polarity of the magnet and the direction of the current, the spiral path being compounded of a rectilinear translation in the direction of the liquid current, and of a rotation in the same sense as the hypothetical Ampèrian currents, or in the opposed sense, according to the sense of the electric current traversing the vein (figs. 3 and 4, Plate XIV.).

The author also attempted to observe the effects produced by powerful magnets upon electrified liquid jets which broke into drops in the air. Here no current proper could traverse the vein; and the electromagnetic effects of the electric convection of the falling drops was so small as to be unappreciable with the apparatus at his disposal.

Since, however, Rowland* has demonstrated that the mechanical motion in space of a charged insulated conductor may affect a magnet, there appears to be every reason to conclude that an electrified liquid jet should obey the same electrodynamic laws as a conductor carrying a current. Indeed the experiments of Mr. Crookes, cited above, prove the analogy to be true in the converse case of the flights of molecules in the ultra-gaseous atmospheres of his exhausted tubes.

So far as the author is aware, there has not yet been made any attempt to apply the same reasonings to the electric convection-streams of the brush-discharge. He ventures to predict that the brush-discharge from a fine point, when taking place at the pole of a powerful magnet, will be found to be subjected to a similar torsion, and to appear spiral in form.

There are recorded in the 'Philosophical Magazine' for 1849 and 1850 some curious researches by Mr. Reuben Phillips on the magnetic behaviour of jets of steam, which are of interest as bearing upon the subject under discussion.

The author has reason to think that the phenomena described in that memoir, and also the much better-known phenomena of the so-called diamagnetism of flames and of smoke-jets, are also only further instances of the magnetic behaviour of electric convection-currents. As, however, this matter will form the subject of a separate research, the author reserves such observations as he might offer on these points for another occasion.

* Phil. Mag. 1876.

LX. *On the Determination of the Variation of the Thermal Conductivity of Metals with Temperature, by means of the permanent Curve of Temperature along a uniform thin Rod heated at one end. (Second Paper*.)*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I WROTE to you last May stating that I had made an oversight in the beginning of the paper on Thermal Conductivity which you published in the March and April Numbers of the Philosophical Magazine, and promising to go through the calculation again and make the necessary correction as soon as I conveniently could.

The slip consisted in setting out with the ordinary equation to the curve of permanent temperature down a rod

$$\frac{d^2\theta}{dx^2} = \frac{Hp}{kq},$$

which is true when k is constant, and working with it as if it were equally valid when k is assumed to be variable. The oversight was inexcusable, because in § 2 of the paper referred to I indicated, for the sake of completeness, the ordinary way in which this fundamental equation is obtained, and I thoughtlessly wrote the gain of heat per second by an element of the rod at a distance x from the origin as

$$kq \frac{d\theta}{dx}$$

as usual, instead of what it obviously becomes when k is not considered constant,

$$dkq \frac{d\theta}{dx};$$

and this slip it is which necessitates my troubling you with the following further communication on the subject, and requires an apology from me both to you and to your readers.

The term containing $\frac{dk}{d\theta}$ which I omitted is but a small one, however, and does not make very much difference to the result: hence the sections 16–20, though superseded by the present communication, are not exactly incorrect, but are first approximations; and the curve A spoken of in § 21, and drawn in Plate X., does represent the character of the curve of temperature down a long iron rod *in vacuo*, with one end

* Communicated by the Physical Society, as a correction of the first paper.

300° hotter than the other. But no calculation of the variation coefficient of conductivity is likely to be possible by means of equations from which the term $\frac{dk}{d\theta}$ had been omitted.

I am, Gentlemen,
Your obedient Servant,
OLIVER J. LODGE.

30. With the exception of the correction now indicated in equations (1) and (3), the first fifteen sections of the paper are quite unaffected by the slip, and may remain as they stand, except that I have now a little more to say on the subject of §§ 5-9.

Professor Tait has been kind enough to send me a copy of his researches on "Thermal and Electric Conductivity," read before the Royal Society of Edinburgh in March and June 1878; and I find that he has given up his enticing speculation as to the inverse variation of thermometric conductivity with absolute temperature—and in fact that he believes iron to be possibly exceptional in the inverse connexion of conductivity and temperature, all other metals which he has subjected to experimental observation showing a slight *increase* of conductivity as the temperature rises. Prof. Tait's results are thus in opposition to the results of Prof. Ångström for copper; but since Prof. Ångström, in the interpretation of his very ingenious method of experiment, used the ordinary Fourier equations, formed on the supposition that k is constant, and that rate of cooling is proportional to excess of temperature, Prof. Tait does not consider his observations competent to decide a point as to the variability of k . Without venturing an opinion of my own on the subject, it is evident that this opposition is an additional reason for attacking the important question of the law of the variation of thermal conductivity with temperature.

Prof. Tait finds that a linear function of the temperature, $k = a + bt$, will express the value of the thermal conductivity according to his experimental results, at least in their present preliminary stage; and we saw in § 8 that Prof. Forbes's results for iron could be expressed nearly as

$$k = \cdot 207(1 - \cdot 00144t);$$

hence instead of the law of variation of *thermometric* conductivity,

$$\frac{k}{cp} = \frac{A}{b+t},$$

assumed in the former paper, I shall now assume as the law of the variation of *thermal* conductivity,

$$k = b\kappa \left(1 + \frac{1}{b} t\right) = \kappa(b + t),$$

where b is positive for metals whose conductivity increases with temperature, but both b and κ are negative for those whose conductivity diminishes with temperature, like iron.

It will be necessary to reckon temperature, as before, from the temperature of the enclosure ($v_0 - 274 = t_0$) as zero; so instead of $b + t$ we write $m + \theta$, where $m = t_0 + b$.

Similarly, for the law of variation of specific heat and density, we may assume (see § 8),

$$c\rho = \beta\sigma \left(1 + \frac{1}{\beta} t\right) = \sigma(\beta + t) = \sigma(n + \theta),$$

where $n = t_0 + \beta$ and is always positive, because the specific heat of all metals except mercury appears to *increase* with temperature.

§§ 10-15 remain unaltered; but I shall only attempt the case of a rod *in vacuo*, as in air the integration appears impracticable. §§ 25-29 also remain practically unaltered.

31. We start, then, with the following equations to the variables, of which the numbering agrees with that of the corresponding equations in the preceding paper, whether they are the same or different; but the numbers are put into square brackets when for any reason the equation is modified. Numbers above 32 characterize those equations which did not appear in the former paper.

$$k = \kappa(m + \theta), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [4]$$

$$\therefore \frac{dk}{d\theta} = \kappa = \frac{k_0}{b} = \frac{k_0}{m + t_0}, \quad . \quad . \quad . \quad . \quad . \quad (33)$$

$$c\rho = \sigma(n + \theta), \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

$$H = \frac{qcp}{p} \dot{\theta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\dot{\theta} = Pa^{v_0}(a^\theta - 1) = R(a^\theta - 1). \quad . \quad . \quad . \quad (5')$$

And the fundamental equation is

$$dkq \frac{d\theta}{dx} = Hpdx; \quad . \quad . \quad . \quad . \quad . \quad [1]$$

or, neglecting the expansion by heat of the cross section q ,

$$\frac{d}{dx} k \frac{d\theta}{dx} = \frac{Hp}{q} = c\rho \dot{\theta}. \quad . \quad . \quad . \quad . \quad [3]$$

This may be written

$$\frac{d^2\theta}{dx^2} + \frac{1}{k} \cdot \frac{dk}{d\theta} \left(\frac{d\theta}{dx} \right)^2 = \frac{c\rho}{k} \dot{\theta},$$

whence, by help of [4] &c.,

$$\frac{d^2\theta}{dx^2} + \frac{1}{m+\theta} \left(\frac{d\theta}{dx} \right)^2 = \frac{\sigma}{\kappa} \cdot \frac{n+\theta}{m+\theta} \cdot \dot{\theta}. \quad [11]$$

32. To integrate this once, make the substitution $\left(\frac{d\theta}{dx} \right)^2 = z$, when [11] becomes

$$\frac{dz}{d\theta} + \frac{2z}{m+\theta} = 2 \frac{\sigma}{\kappa} \cdot \frac{n+\theta}{m+\theta} \cdot \dot{\theta},$$

which is of the form

$$\frac{dz}{d\theta} + Pz = Q,$$

whose integral is

$$z = e^{-\int P d\theta} \left\{ \int e^{\int P d\theta} \cdot Q d\theta + C \right\}.$$

Hence the first integral of [11] is

$$\left(\frac{d\theta}{dx} \right)^2 = \frac{2\sigma}{\kappa(m+\theta)^2} \int_0^\theta (m+\theta)(n+\theta) \dot{\theta} d\theta,$$

the limits being taken to suit a rod whose length is unlimited (that is, one in which θ and $\frac{d\theta}{dx}$ vanish together)—a condition which is necessary for simplicity, as is explained in § 17.

We may now insert the value of $\dot{\theta}$ from (5) and write the above thus,

$$\begin{aligned} \frac{\kappa}{2R\sigma} \left\{ (m+\theta) \frac{d\theta}{dx} \right\}^2 &= \int_0^\theta (m+\theta)(n+\theta)(a^\theta - 1) d\theta \\ &= \frac{a^\theta - 1}{\log a} \{ M + (mn - M)\theta + \theta^2 \} - M\theta, \quad [12] \end{aligned}$$

where for shortness the letter M is written instead of the constant

$$mn - \frac{m+n}{\log a} + \frac{2}{(\log a)^2}.$$

33. So far we have proceeded with perfect accuracy; but in order to integrate the equation any further it is necessary to expand $a^\theta - 1$ and neglect higher powers of $\theta \log a$, as was done and justified for all probable values of θ in § 12. The

approximation used is

$$a^\theta - 1 \doteq \theta \log a \left\{ 1 + \frac{1}{2} \theta \log a + \frac{1}{6} (\theta \log a)^2 (1 + \frac{1}{8} \Theta \log a) \right\}. \quad (7')$$

Introducing this into [12], after writing γ for the small correction factor $\frac{1}{8} \Theta \log a$, and α for the perpetually occurring constant $\log a = \log_e 1.0077 = \frac{1}{130}$, it becomes

$$\begin{aligned} & \frac{\kappa}{2R\sigma} \left\{ (m + \theta) \frac{d\theta}{dx} \right\}^2 = \\ & = \theta \left(1 + \frac{1}{2} \alpha \theta + \frac{1 + \gamma}{6} \alpha^2 \theta^2 \right) (M + (mn - M)\theta + \theta^2) - M\theta \\ & = \frac{1}{2} \alpha \theta^2 (A + B\theta + C\theta^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [13] \end{aligned}$$

where

$$\left. \begin{aligned} A &= \frac{1}{\alpha} \left(\alpha mn + m + n - \frac{2}{\alpha} \right), \\ B &= \frac{2 - \gamma}{3} (m + n) + \frac{1 + \gamma}{3} \left(\alpha mn + \frac{2}{\alpha} \right), \\ C &= 1 + \frac{1 + \gamma}{3} \left(m + n - \frac{2}{\alpha} + \theta \right) \alpha. \end{aligned} \right\} \quad (35)$$

(Remember that $\frac{1}{\alpha} = 130$, and that $\gamma = \frac{1}{8} \alpha \Theta = \frac{\Theta}{1000}$ practically.)

The coefficient C, therefore, is not quite constant, but depends upon θ . The dependence, however, is very slight, since $m + n$ is usually a large number, and α a small fraction; and it will be quite sufficient to write the average value $\frac{1}{2} \Theta$ instead of θ in the brackets of C, and thus to make it a constant. The usual relative sizes of the three constants are, A numerically much larger than B, and B numerically much larger than C.

For metals whose conductivity increases with temperature all three constants are positive; but for metals like iron, whose conductivity decreases with temperature and for which therefore m is negative, A and B are certain to be negative, while C is very likely to be positive but small. We can avoid this change of sign by noticing that when m is negative κ is also negative; hence, if we bring κ over to the right-hand side of [13], we shall get new constants, $\frac{A}{\kappa}$ and $\frac{B}{\kappa}$, which will be always positive, and $\frac{C}{\kappa}$, which is positive for all metals which have κ positive, but which is generally negative for those which, like iron, have κ negative. In all the following equations, where A and B appear alone, they may be always reckoned positive, because the κ has merely been cancelled out. In

order that C may be negative, it is necessary not only that m shall be negative, but also that it shall be numerically greater than $n + \frac{1}{2}\Theta + \frac{1}{\alpha}$; hence this is the condition which will allow $\frac{C}{\kappa}$ to be positive when κ is negative.

34. It may be useful to calculate the numerical value of these constants for such metals as we at present possess any experimental data for. We will assume the temperature of the enclosure to be 0°C. (so that $m=b$ and $n=\beta$, see § 30), and the highest observed point of temperature Θ on the rod to be 100°C. : then the following Table contains the values of the constants A , B , and C for iron and copper, together with certain ratios which will be used later. The row of numbers deduced from the experiments of Forbes, confirmed by Tait, are probably nearly accurate; the others are subject to discount.

Metal.	Value of β or n according to Bède and Fizeau.	Value of b or m according to		A.	B.	C.
Iron ...	760	Forbes ...	— 700	— 578,000	— 1,360	+ .577
		Ångström	— 640	— 504,600	— 1,200	+ .746
		Ångström	— 940	— 1,964,000	— 5,063	+ 3.4
Copper..	2200	Tait	+2000	+4,912,200	+15,166	+12.3

$\frac{2A}{B}=r.$	$\frac{2C}{B}=s.$	$rs=\frac{4AC}{B^2}.$
+850	— .00085	— .7225
+841	— .00124	— 1.0428
+775.7	— .00134	— 1.0394
+647.8	+ .00162	+ 1.0494

35. The integral of equation [13] may be written down without difficulty; and it constitutes the equation to the permanent curve of temperature down a long thin uniform cylindrical metal rod with a blackened surface heated at one end *in vacuo*.

$$\left[\log \frac{\left\{ \frac{2A}{\theta} + B + 2\sqrt{A}\sqrt{\left(\frac{A}{\theta^2} + \frac{B}{\theta} + C\right)} \right\}^{m\sqrt{\frac{\kappa}{A}}}}{\left\{ 2C\theta + B + 2\sqrt{C}\sqrt{(A + B\theta + C\theta^2)} \right\}^{\sqrt{\frac{\kappa}{C}}}} \right]_{\theta} = \sqrt{(R\sigma\alpha)x}. [14]$$

For a much simpler and practically useful form of this equation see equation (40) § 38.

36. The expression on the left-hand side of this equation may be written in various ways; and its form is slightly different according as C is of the same or opposite sign to κ .

For the case when C and κ have the same sign (which according to Prof. Tait is probably most usual) we may write it conveniently

$$\left[m \sqrt{\left(\frac{\kappa}{A}\right)} \sinh^{-1} \frac{\frac{2A}{\theta} + B}{\sqrt{(4AC - B^2)}} - \left(\frac{\kappa}{C}\right) \sinh^{-1} \frac{2C\theta + B}{\sqrt{(4AC - B^2)}} \right]_{\theta}^{\theta} = \sqrt{(R\sigma\alpha)x}. \quad (36)$$

But for the case when the signs of C and κ are opposite (as for iron), it becomes

$$\left[m \left(\frac{\kappa}{A}\right) \cosh^{-1} \frac{\frac{2A}{\theta} + B}{\sqrt{(B^2 - 4AC)}} - \sqrt{\left(\frac{-\kappa}{C}\right)} \cos^{-1} \frac{2C\theta + B}{\sqrt{(B^2 - 4AC)}} \right]_{\theta}^{\theta} = \sqrt{(R\sigma\alpha)x}. \quad (37)$$

Whether we write $\cos^{-1}$ or $\sin^{-1}$ in this equation only affects the sign of the term containing it. In equation (36), if $4AC$ is less than B^2 (which is unlikely), the terms in the denominators must be transposed and $\cosh^{-1}$ written for $\sinh^{-1}$.

Of the two terms in the brackets of these equations the first is by far the most important, and in the former paper is the only one which appeared (see equation 14); the second term has only a small effect on the curve, and this effect vanishes with C^* . The occurrence of the inverse circular function in the curve of temperature along metals whose conductivity diminishes with temperature is peculiar; but C must always be very small for such metals; so that it does not make much difference. And this is a good thing, because when m is negative the θ occurring in the expression called C is of more relative importance, and C is therefore not so constant as when m is positive: but the approximation is always pretty good, the worst possible case being that of a supposititious metal with $m = -\left(n - \frac{2}{\alpha}\right)$, for which $C = 1 + \frac{\theta}{355}$.

37. We have thus obtained the equation to the curve of temperature expressing x as a function of θ . What we have now to do is to show how, from experimentally observed cor-

* To avoid a possible misunderstanding, it may be well to say that this does not mean that the term vanishes, because of course it becomes infinite; but it means that the effect of the term vanishes, because when the limits are put in, the two things subtracted from one another are equal.

responding values of θ and x , the constants A, B, C can be determined, or at least such of them as are required for the determination of the constants κ and m . In order to determine these constants we must apparently use some method of successive approximations; and the precise method adopted will probably be a matter of taste. I may, however, suggest the following as certainly applicable to the case of κ and m positive, *i. e.* to equation (36), and as inferentially applicable to the other case (37) if we can get some imaginary quantities to cancel each other. We will therefore proceed with the general case, and not trouble about whether the quantities are real or imaginary.

$$\frac{2A}{\theta} + B$$

The quantity $\frac{\frac{2A}{\theta} + B}{\sqrt{(4AC - B^2)}}$ is always pretty large, even when θ has its maximum value Θ ; call it y . Then writing $\sinh^{-1} y = \log(y + \sqrt{y^2 + 1})$, we see that, since y is large, $y^2 + 1$ is practically the same as y^2 , and therefore that $\sinh^{-1} y \doteq \log 2y$ to all intents and purposes. This approximation is always very close; and it is perfectly accurate when $rs = 1$, *i. e.* when $4AC = B^2$. That this is not likely to be far from the case is illustrated in the Table (§ 34).

The quantity $\frac{2C\theta + B}{\sqrt{(4AC - B^2)}}$, however, is not large at all, but has a value not very different from unity. We had better therefore take its $\sinh^{-1}$ in the logarithmic form, and write (36) with the limits put in

$$m \sqrt{\frac{\kappa}{A} \log \frac{\frac{2A}{\theta} + B}{\frac{2A}{\Theta} + B}} - \sqrt{\frac{\kappa}{C} \log \frac{2C\theta + B + \sqrt{4AC + 4BC\theta + 4C^2\theta^2}}{2C\Theta + B + \sqrt{4AC + 4BC\Theta + 4C^2\Theta^2}}} = \sqrt{(R\sigma\alpha)x} \quad \dots \quad (38)$$

or, writing $\frac{2A}{B} = r$ and $\frac{2C}{B} = s$,

$$\left[\frac{1 + \frac{r}{\theta}}{1 + \frac{r}{\Theta}} \right] \cdot \left[\frac{1 + s\Theta + \sqrt{(rs + 2s\Theta + s^2\Theta^2)}}{1 + s\theta + \sqrt{(rs + 2s\theta + s^2\theta^2)}} \right]^{\frac{1}{m} \sqrt{\frac{A}{C}}} = \frac{1}{e^m} \sqrt{\left(\frac{AR\sigma\alpha}{\kappa} \right)_x}, \quad \dots \quad (39)$$

or

$$\left(1 + \frac{r}{\theta} \right) (1 + s\theta + \sqrt{rs + 2s\theta + s^2\theta^2})^{-\frac{1}{m} \sqrt{\frac{r}{s}}} = K e^{\mu x}; \quad [19]$$

and the function of θ on the left-hand side of this equation increases in geometrical progression for an arithmetical increase of x .

38. So far we have been practically exact; but it is now necessary to introduce several approximations. Notice that r is a large constant and s a small one; so that $\frac{\theta}{r}$ and $s\theta$ are small quantities whose squares may be neglected for all probable values of θ . Moreover observe that $\frac{1}{m}\sqrt{\frac{r}{s}}$, though it may be imaginary, is never large and is often fractional. Write, therefore, the denominator of the left-hand side of [19] in the following successively approximate forms,

$$\begin{aligned} & \left[1 + s\theta + \sqrt{rs} \sqrt{1 + \frac{2\theta}{r}} \right]^{\frac{1}{m}\sqrt{\frac{r}{s}}} \\ & \doteq \left[1 + s\theta + \sqrt{rs} \left(1 + \frac{\theta}{r} \right) \right]^{\frac{1}{m}\sqrt{\frac{r}{s}}} \\ & = \left[1 + \sqrt{rs} + \theta \left(s + \sqrt{\frac{s}{r}} \right) \right]^{\frac{1}{m}\sqrt{\frac{r}{s}}} \\ & \doteq (1 + \sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}} + \frac{1}{m} \theta (\sqrt{rs} + 1) (1 + \sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}} \left(\frac{r}{s} \right)^{-1} \\ & = \frac{m + \theta}{m} (1 + \sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}}. \end{aligned}$$

Hence we have as the quantity which is to go in geometrical progression in equation [19]

$$\frac{1 + \frac{r}{\theta}}{m + \theta} = \frac{(1 + \sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}}}{m} \cdot K e^{\mu x}, \quad [19']$$

and we may conveniently write equation [14] to the curve of temperature down the rod in the simple approximate form

$$\theta \cdot \frac{m + \theta}{r + \theta} = \Theta \cdot \frac{m + \Theta}{r + \Theta} e^{-\mu x}, \quad (40)$$

where it is to be remembered, see equation (39), that

$$\mu^2 = \frac{AR\sigma\alpha}{m^2\kappa} \doteq * \frac{mn\sigma R\alpha}{m^2\kappa} = \frac{c_0\rho_0}{k_0} R\alpha = \frac{c_0\rho_0}{k_0} \cdot Pa^{\tau_0} \log a, \quad [21]$$

* The approximation in [21] consists in writing mn , the largest term of A , instead of A . It is unnecessary to work with this approximation; but it is useful as showing how naturally the various constants occur in μ .

and that

$$r = \frac{2A}{B} = \frac{6}{\alpha} \cdot \frac{\alpha mn + m + n - \frac{2}{\alpha}}{(2-\gamma)(m+n) + (1+\gamma)\left(\alpha mn + \frac{2}{\alpha}\right)}. \quad (41)$$

39. There ought now to be no difficulty in calculating the constant m from observed corresponding values of θ and x . The following method has occurred to me; but there may be better ones. Let five temperatures, $\theta_0, \theta_1, \theta_2, \theta_3, \theta_4$, be observed along the rod at distances from the origin $x, x+\xi, x+2\xi, x+3\xi$, and $x+4\xi$. Let the quantity which goes in geometrical progression, see equations (40) or [19'], be written

$$\frac{1 + \frac{r}{\theta}}{1 + \frac{r}{m}};$$

and for shortness call the numerator $\phi(\theta)$ and the denominator $\psi(\theta)$. Then of course

$$\frac{\phi_1}{\psi_1} \cdot \frac{\phi_3}{\psi_3} = \left(\frac{\phi_2}{\psi_2}\right)^2,$$

whence

$$\frac{\phi_1\phi_3 - \phi_2^2}{\phi_2^2} = \frac{\psi_1\psi_3 - \psi_2^2}{\psi_2^2}.$$

Similarly

$$\frac{\phi_0\phi_4 - \phi_2^2}{\phi_2^2} = \frac{\psi_0\psi_4 - \psi_2^2}{\psi_2^2}.$$

Therefore

$$\frac{\phi_1\phi_3 - \phi_2^2}{\phi_0\phi_4 - \phi_2^2} = \frac{\psi_1\psi_3 - \psi_2^2}{\psi_0\psi_4 - \psi_2^2},$$

which, being interpreted, is

$$\frac{\frac{1}{\theta_1} + \frac{1}{\theta_3} - \frac{2}{\theta_2} + r\left(\frac{1}{\theta_1\theta_3} - \frac{1}{\theta_2^2}\right)}{\frac{1}{\theta_0} + \frac{1}{\theta_4} - \frac{2}{\theta_2} + r\left(\frac{1}{\theta_0\theta_4} - \frac{1}{\theta_2^2}\right)} = \frac{(\theta_1 + \theta_3 - 2\theta_2)m + \theta_1\theta_3 - \theta_2^2}{(\theta_0 + \theta_4 - 2\theta_2)m + \theta_0\theta_4 - \theta_2^2}, \quad \dots \dots (42)$$

the form of which we may abridge into

$$\frac{a_1 + rb_1}{a_0 + rb_0} = \frac{mc_1 + d_1}{mc_0 + d_0},$$

where the coefficients a, b, c, d are all known. This gives us

$$m = \frac{a_0 d_1 - a_1 d_0 + r(b_0 d_1 - b_1 d_0)}{a_1 c_0 - a_0 c_1 + r(b_1 c_0 - b_0 c_1)}. \quad (43)$$

40. Now looking at equation (41), we see that the value of r is $\frac{6}{\alpha}$, multiplied by a fraction which contains m indeed, but does not depend very much upon it; for, neglecting the small quantity γ , the fraction is

$$\frac{\alpha mn + m + n - \frac{2}{\alpha}}{\alpha mn + 2(m + n) + \frac{2}{\alpha}};$$

and since m and n are both pretty large, the first term, which is the same in both numerator and denominator, is much the biggest, and accordingly the fraction is not very different from unity. It is likely to be greater than 1 when m is negative, and less than 1 when m is positive. Hence a first approximation to r is $\frac{6}{\alpha}$, or 780. The fact that r does not depend much upon m is apparent in the Table, § 34.

Making, then, a guess at r as 800 or so, we obtain from (43) a first approximation to m ; and we can afterwards improve it by trial and error, so that the quantity $\theta \frac{m + \theta}{r + \theta}$ in equation (40) really does go in geometrical progression down the whole length of the rod—for instance, so that

$$\frac{1}{x} \log \frac{\Theta(m + \Theta)(r + \theta)}{\theta(m + \theta)(r + \Theta)} = \text{const} = \mu. \quad (44)$$

And then, having obtained the mean value of this constant μ , it is easy to calculate the absolute conductivity of the rod from [21] if one has determined R by experiments on cooling (see § 27). The *relative* thermometric conductivities of different metals at the temperature of the enclosure are simply inversely as μ^2 (see [21]).

41. The only experimental results already published which are even apparently at all suitable for applying the method to are those of Wiedemann and Franz; but it is impossible to get any results from these for reasons stated in § 28. It may, however, be interesting to see how far their numbers for an iron rod in a vacuum will lend themselves to the equations now obtained, if m is assumed to be -700 and r to be 850 , as in § 34.

The temperatures of the rod were read thermoelectrically, and stated in terms of galvanometer-deflections; and though a little Table is given showing what these deflections experimentally mean in Centigrade excess of temperature, the numbers actually used in their calculations are the deflections themselves, which are only roughly proportional to the temperature excesses. The following empirical relation between δ (the deflection) and θ (the excess of temperature) is deduced from their little comparison Table,

$$\theta = \frac{1}{4} \left(\delta + 2 - \frac{\delta}{10 - \frac{\delta}{100}} \right);$$

and this I have used to obtain the second column of the following Table:—

Iron (Wiedemann and Franz).

Galvanometer deflections. δ .	Excess of temperature in Centigrade degrees. θ .	$\log_{10} \left(\theta \frac{700 - \theta}{850 + \theta} \right)$ $= \log f(\theta)$.	$\frac{1}{n-1} \log \frac{f(\theta_1)}{f(\theta_n)}$ $= \mu \xi \log_{10} e$.	$\frac{f(\theta_{n-1}) + f(\theta_{n+1})}{f(\theta_n)}$ $= 2 \cosh \mu \xi$.
230	50.8	1.5637		
153 $\frac{1}{2}$	34.5	1.4142	.1495 ^a	2.094 ^b
100 $\frac{1}{2}$	22.9	1.2487	.1575	2.168
67 $\frac{1}{2}$	15.8	1.0965	.1557	2.068
42	10.1	.9086	.1638	2.172
25.7	6.3	.7079	.1712	2.147
13.2	3.5	.4558	.1846	

^a The mean of the first four numbers in this column is .1566; therefore
 $\mu \xi = .3606$.

^b The mean of these numbers is 2.130; therefore
 $\mu \xi \triangleq \sqrt{(2 \cosh \mu \xi - 2)} = .3606$.

The third column contains the logarithm of the quantity which ought to go in geometrical progression. The fourth column shows that this law of progress is moderately true for the first four numbers, but that the numbers which ought to be constant exhibit a decided increase towards the cooler end of the bar, probably because the bar was so short that the flow of heat along it extended through the point where $\theta=0$ —which is contrary to our hypothesis (§§ 32 and 17) that $\frac{d\theta}{dx}$ shall vanish with θ . The distance ξ was 2.6 centimetres.

The next Table shows their results for silver tabulated in the same way, and on the assumption that $m = +1000$.

Silver (Wiedemann and Franz).

δ .	θ .	$\log_{10} \left(\theta \frac{1000+\theta}{700+\theta} \right)$	$\log f(\theta_1) - \log f(\theta_n)$	$f(\theta_{n-1}) + f(\theta_{n+1})$
		$= \log f(\theta)$.	$n-1$ $= \mu \xi \log e$.	$f'(\theta^n)$ $= 2 \cosh \mu \xi$.
194	42.5	1.7758		
167	37.3	1.7200	.0558 ^a	1.977 ^b
142	32.0	1.6544	.0607	2.024
122	27.5	1.5893	.0621	2.018
104	23.5	1.5217	.0635	2.030
88½	20.2	1.4568	.0628	2.036
75½	17.7	1.3994	.0627	

^a The mean of these numbers is .0614; hence

$$\mu \xi = \frac{.0614}{.4343} = .141.$$

^b The mean of these is 2.017; hence

$$\mu \xi = \sqrt{.017} = .1304.$$

42. Although the whole investigation applies only to the case of a long rod, yet it seems extremely possible that something very like the proper equation to the curve of temperature down a short rod with given temperature at its two ends can be written down from equation (40) by the addition of another term to the right-hand side, thus,

$$\theta \frac{m+\theta}{r+\theta} = A e^{\mu x} + B e^{-\mu x}; \quad . \quad . \quad . \quad (45)$$

just as the ordinary equation for an infinite rod

$$\theta = \Theta e^{-\mu x}$$

becomes

$$\theta = A e^{\mu x} + B e^{-\mu x}$$

for a short one. If so, the conductivity-constant μ and the variation-constant m would be best determined from the relation

$$\frac{\theta_1 \frac{m+\theta_1}{r+\theta_1} + \theta_3 \frac{m+\theta_3}{r+\theta_3}}{\theta_2 \frac{m+\theta_2}{r+\theta_2}} = \text{const} = 2 \cosh \mu \xi \quad . \quad . \quad [25]$$

by some such process as is given in §§ 23–25.

The extent of the constancy of this quotient with Wiedemann's numbers is therefore exhibited in the last column of the preceding tables; and the value of $\mu \xi$ is obtained from this

column in order to compare it with that obtained from the column preceding. In the case of iron the two values accidentally agree exactly.

43. As regards the concluding section of the former paper (the suggestions for future experiment), I have seen no reason to modify it in any way. I still think the rods should be examined by a thermoelectric compensation method (*i. e.* with the use of a bridge wire and slider, and with fixed thermoelectric joints on the rod) after their surfaces have been uniformly coated with stove black-lead. The exhaustion of the chamber containing the rod should proceed to the "neutral-point" observed by Mr. Crookes, and employed by Mr. Poynting in his delicate weighing experiment (Proc. Roy. Soc.), "when convection currents have ceased and the radiometer effect has not yet begun." It is probable that at such a point (if it really exists, as I suppose it does) Dulong and Petit's law of cooling, assumed all through this paper, would hold with perfect accuracy; and I think that with very careful observations of temperature over a sufficient range on long thin rods, there would be no insuperable obstacle to obtaining rapidly the absolute conductivity and the coefficient of its variation with temperature of as many metals and alloys as one pleased.

I have taken considerable care; and I hope that all the calculations in the present paper are free from error; but they are rather long and unsymmetrical and therefore tiresome, and I have not ventured to ask any one to read over the proof for this very reason.

LXI. *Researches on the Elementary Law of Hydrodiffusion.*

By H. F. WEBER.

[Concluded from p. 500.]

Second Method.

Description.—The above-described first method requires rather long periods of time for the observations, in consequence of which the investigation is very tedious and, on account of the absolute necessity of a constant temperature, in most localities impracticable. It certainly appears possible to shorten the duration of the observations considerably by diminishing the length L ; but L can scarcely be allowed to fall much below the value employed in the above experiments if the initial state of the diffusion-vessel is to remain sharply definable.

This property of the method used induced me to seek other

methods of investigation ; and I succeeded in discovering one which affords the possibility of *making within a few hours an extremely delicate trial of the correctness of Fick's elementary law of diffusion, and determining within any fraction of an hour the quantity of the constant of diffusion.* By this procedure the influences of the temperature and concentration upon the course of the diffusion can be investigated in a way as much more exact as it is more convenient. It is as follows :—

A plane, circular, amalgamated zinc plate forms the bottom of a shallow glass cylinder about 12 centims. wide. Upon three places at the margin of this plate bits of a plane-parallel hard-gum plate, each of 0.52 centim. thickness, are put, which have to support a second amalgamated zinc plate exactly similar to the lower one. The slight interspace between the two parallel zinc plates is filled up with a solution of zinc sulphate of any degree of concentration : in the following experiments concentrations between 0.20 and 0.38 were employed. Wires are soldered to the two zinc plates in order to send a galvanic current through the system consisting of the plates and the the solution, or to insert it in the circuit of a galvanometer. If before the experiment the two zinc plates are rubbed down and then rinsed with solution of zinc sulphate of the same concentration as that which is to fill the interspace between them, the system of plates and solution shows itself absolutely without any difference of potential when both plates have the same temperature ; only when the two have not exactly the same temperature does a slight deflection appear on a sensitive galvanometer in the circuit of which the system is inserted. Hereupon a galvanic current, maintained constant, is sent through the combination thus prepared, entering through the lower plate and issuing through the upper one. By the migration of the ions the salt-solution becomes more concentrated in the boundary layer at the lower zinc plate (the anode), and more dilute in the boundary layer at the upper (the cathode). These alterations of concentration of the boundary layers are proportional to the intensity and the duration of the current. Now, as soon as through the action of the galvanic current these alterations of concentration commence, the diffusion-current develops itself, which tends to counterbalance the action of the galvanic current. By the cooperation of the galvanic and the diffusion-current a stationary condition of the distribution of concentration between the two electrodes is striven after, in which exactly as much salt is carried in unit time by the diffusion $\left\{ \begin{array}{l} \text{to} \\ \text{from} \end{array} \right\}$

the $\left\{ \begin{smallmatrix} \text{upper} \\ \text{lower} \end{smallmatrix} \right\}$ boundary layer as is $\left\{ \begin{smallmatrix} \text{withdrawn from} \\ \text{communicated to} \end{smallmatrix} \right\}$ the $\left\{ \begin{smallmatrix} \text{upper} \\ \text{lower} \end{smallmatrix} \right\}$ boundary layer by the galvanic current.

When the constant galvanic current has passed through the combination a suitably long time, and if an exactly specifiable state of concentration-distribution between the two electrodes has in consequence arisen, the galvanic current is suddenly interrupted. From this moment the solution is exposed to the action of diffusion only. The further course of the diffusion is now investigated with the aid of the electromotive force which is generated by the variable concentrations of the boundary layers at the two zinc plates. From the found temporal course of this electromotive force both the entire course of the diffusion can be known and the diffusion-constant calculated.

An eminent advantage of this method consists in the circumstance that, the distance between the electrodes, and consequently the time occupied by the entire course of the diffusion may be made *as short as we please*, without introducing any uncertainty into the definition of the initial state of the distribution of concentration from which the observed diffusion is developed; at whatever moment the galvanic current may be interrupted, the distribution of the concentration from layer to layer corresponding to that moment can always be assigned.

Theory.—When a galvanic current passes through a zinc-sulphate solution between two zinc electrodes, the quantity of salt remains unchanged, in spite of the electrolysis that takes place; just so the concentration of every single inner layer of liquid remains constant; only the boundary layers of the solution, situated at the electrodes, undergo an alteration of their concentration: the salt-content of the boundary layer adjacent to the anode is augmented; the salt-content of that at the cathode is diminished. The quantity of salt which a constant current of intensity I , through the processes of electrolysis and the migration of the ions, brings during the time t to the boundary of the anode, and carries away from the boundary at the cathode, is, according to Hittorf's comprehensive investigations,

$$2.48 (1 - n) \epsilon I t,$$

where n denotes the transport-number and ϵ the electrochemical equivalent of zinc. It follows from Hittorf's measurements that n for high concentrations, and within a not too great

interval of the concentration, varies scarcely perceptibly with the concentration. The concentration used in the following experiments possessed such high values, and the differences of concentration produced were kept within such narrow limits, that the value of n could not vary more than a fraction per cent.

In order to ascertain the connexion subsisting between the concentration z in any layer at the depth x below the upper electrode after the lapse of time t from the commencement of the current and these two variables x and t , let us start from the hypothesis that, if the action of the galvanic current at all influences the progress of the diffusion, this influence asserts itself only in the numerical value of the diffusion-constant k . (I may here remark that a trial of the correctness of this hypothesis, mentioned further on, showed that the galvanic current passing through the solution simultaneously with the diffusion did not in any way perceptibly influence the latter.) The variable concentration z has then first to satisfy the partial differential equation

$$\frac{\partial z}{\partial t} = k \frac{\partial^2 z}{\partial x^2} \cdot \cdot \cdot \cdot \cdot \cdot \quad (1)$$

Moreover z has to satisfy two limiting conditions, the form of which is first to be developed. This development may be carried out for the one boundary surface of the salt-solution at $x=0$:—

From the boundary layer in contact with the upper electrode (the cathode), of the infinitesimal thickness dx , the galvanic current of intensity I carries away during the element of time dt the quantity of salt $hI dt$, if the symbol h be put for the constant $2.48(1-n)\epsilon$. If the values of the differential quotient of the concentration in the direction of x for the place $x=0$ be denoted by $\left(\frac{\partial z}{\partial x}\right)_0$, $\left(\frac{\partial^2 z}{\partial x^2}\right)_0$, then the expression of the quantity of salt brought by the diffusion-current, during the same time, to this layer, through its lower boundary surface, from the deeper-lying layers, is

$$kq \left\{ \left(\frac{\partial z}{\partial x}\right)_0 + \left(\frac{\partial^2 z}{\partial x^2}\right)_0 dx \right\} dt,$$

where q signifies the cross section of the diffusion-vessel. The quantity of salt gained by the infinitely thin boundary layer during the time-element dt is therefore

$$\left\{ kq \left(\frac{\partial z}{\partial x}\right)_0 - hI \right\} dt + kq \left(\frac{\partial^2 z}{\partial x^2}\right)_0 dx dt.$$

As, on the other hand, this gain of salt can also be represented by the value $q \left(\frac{\partial z}{\partial t} \right)_0 dx dt$, the following equation holds good as the expression of the salt-motion in the boundary layer at $x=0$:—

$$q \left(\frac{\partial z}{\partial t} \right)_0 dx dt = \left\{ kq \left(\frac{\partial z}{\partial x} \right)_0 - hI \right\} dt + kq \left(\frac{\partial^2 z}{\partial x^2} \right)_0 dx dt.$$

For this layer must therefore, at every moment,

$$kq \left(\frac{\partial z}{\partial x} \right)_0 = hI \quad . \quad . \quad . \quad . \quad . \quad (2)$$

for all values of t . In this case, therefore, a limit-equation is valid which is analogous to the well-known one in the theory of the conduction of heat.

By applying the same manner of consideration to what takes place in the motion of the salt in the lower boundary layer (in contact with the anode), we get as a second limiting equation, valid at every instant,

$$kq \left(\frac{\partial z}{\partial x} \right)_L = hI. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

for all values of t .

If as the initial point of time that instant be taken in which the galvanic current begins to pass through the solution, the initial equation will have the form

$$z = z_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

for $t=0$ and for all values of x . A solution which satisfies equations (1), (2), and (3) is

$$z = \frac{Ih}{kq} x + n \sum_0^{\infty} A_n \cos \left(\frac{n\pi}{L} x \right) e^{-\frac{n^2 \pi^2}{L^2} kt},$$

where $n=0, 1, 2 \dots$

There still remains so to determine the constant A_n as to satisfy the initial condition (4). From the equation for $t=0$,

$$z_0 = \frac{Ih}{kq} x + n \sum_0^{\infty} A_n \cos \left(\frac{n\pi}{L} x \right),$$

it follows that

$$A_0 = z_0 - \frac{Ih}{kq} \frac{L}{2}$$

and

$$A_n = \frac{4}{\pi^2} \cdot \frac{Ih}{kq} \cdot \frac{L}{n^2} \cdot \sin \left(\frac{n\pi}{2} \right);$$

therefore

$$A_2 = A_4 = A_6 = \dots = 0.$$

Consequently the general solution, fulfilling all the conditions, of the present problem is

$$z = z_0 + \frac{Ih}{kq} \left(x - \frac{L}{2} \right) + \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ \cos\left(\frac{\pi x}{L}\right) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} \cos\left(\frac{3\pi}{L} x\right) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\}. \quad (5)$$

From this we get for the concentrations z'' and z' (present in the boundary layers for $x=L$ and $x=0$ respectively) the values

$$z'' = z_0 + \frac{Ih}{kq} \cdot \frac{L}{2} - \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\},$$

$$z' = z_0 - \frac{Ih}{kq} \cdot \frac{L}{2} + \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\},$$

and the electromotive force E , present at the instant t between the two zinc electrodes, has the quantity

$$E = A(z'' - z')[1 + B(z'' + z')]$$

$$= A[1 + 2Bz_0] \frac{Ih}{kq} L \left\{ 1 - \frac{8}{\pi^2} \left(e^{-\frac{\pi^2}{L^2} kt} + e^{-\frac{9\pi^2}{L^2} kt} + \dots \right) \right\}. \quad (6)$$

With the aid of this equation (6) the correctness of the elementary law of diffusion might be tested and the value of the constant k determined; but a closer discussion of this equation enables us to perceive that its peculiar form does not permit any very accurate determination of the quantity k ; a very small error of observation in the measurement of the electromotive force E has even a proportionally great influence upon the value of the constant to be determined. On this ground I have not made use of equation (6) for the definitive measurement of the course of diffusion. I have, however, employed it for the decision of the following, in many respects interesting question:—Does the galvanic current passing simultaneously with the diffusion-current through the salt-solution possess any influence over the course of the diffusion (*i. e.* over the magnitude of the diffusion-constant), or not? Repeated series of experiments proved that the quantity of the diffusion-constant determined by aid of equation (6) as good as perfectly agreed with the value obtained for it from what took place when the diffusion proceeded without the simultaneous passage of a galvanic current through the solution.

From equation (5), however, an extremely convenient method for the investigation of the course of diffusion is obtained in the following manner:—

When the constant galvanic current has traversed the salt-solution during a suitable long time, say during the time T , the current is to be interrupted. The differences of concentration produced in the different layers by the simultaneous action of the current and the diffusion will from that instant gradually be equalized by the diffusion proceeding alone. This gradual equalization can be followed with extraordinary accuracy, and, on that account, can serve as one of the finest means of testing the correctness of Fick's elementary law of diffusion.

The law according to which this equalization proceeds can easily be ascertained. In every place $x > 0$ and $< L$, and at any time, the following differential equation is to be satisfied:—

$$\frac{\partial z}{\partial t} = k \frac{\partial^2 z}{\partial x^2} \quad \dots \quad (7)$$

For all moments of time t the limiting equations

$$\left(\frac{\partial z}{\partial x} \right)_{x=0} = 0 \quad \dots \quad (8)$$

and

$$\left(\frac{dz}{dx} \right)_{x=L} = 0 \quad \dots \quad (9)$$

subsist. As the initial point of time we will take the instant at which the current was interrupted. Let the value of the concentration which is present at that point of time in any layer at the depth x below the upper electrode be z_0^* . The initial state of the diffusion-process is then determined by the equation, For $t=0$,

$$z_0^* = z_0 + \frac{h}{kq} \left(x - \frac{L}{2} \right) + \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ B_1 \cos \left(\frac{\pi}{L} x \right) + \frac{1}{9} B_3 \cos \left(\frac{3\pi}{L} x \right) + \frac{1}{25} B_5 \cos \left(\frac{5\pi}{L} x \right) + \dots \right\}, \quad (10)$$

in which it is supposed that

$$B_1 = e^{-\frac{\pi^2}{L^2} kT}, \quad B_3 = e^{-\frac{9\pi^2}{L^2} kT}, \dots$$

The discovery of the general solution for z , which satisfies all the conditions, presents no difficulty. As giving the calculation would only be a repetition of what has already been

done, I will at once give the final result. The general solution is

$$z = z_0 - \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ (1 - B_1) \cos\left(\frac{\pi}{L} x\right) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} (1 - B_3) \cos\left(\frac{3\pi}{L} x\right) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\}. \quad (11)$$

Thence is found for the concentration of the boundary layer at the lower electrode

$$z'' = z_0 + \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ (1 - B_1) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} (1 - B_3) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\},$$

and for the concentration at the upper electrode

$$z' = z_0 - \frac{4}{\pi^2} \frac{Ih}{kq} L \left\{ (1 - B_1) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} (1 - B_3) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\}.$$

Since the difference of these concentrations

$$z'' - z' = \frac{8}{\pi^2} \frac{Ih}{kq} L \left\{ (1 - B_1) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} (1 - B_3) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\}$$

and their sum

$$z'' + z' = 2z_0,$$

the expression of the electromotive force E which is present at the instant t between the two electrodes takes the form

$$\begin{aligned} E &= A(z'' - z')[1 + B(z'' + z')] \\ &= A(1 + 2Bz_0) \frac{8}{\pi^2} \frac{Ih}{kq} L \left\{ (1 - B_1) e^{-\frac{\pi^2}{L^2} kt} + \frac{1}{9} (1 - B_3) e^{-\frac{9\pi^2}{L^2} kt} + \dots \right\}. \end{aligned} \quad (12)$$

After the lapse of a certain time the value of the second term becomes insignificant in comparison with that of the first. From that moment the following stands as the expression for the electromotive force,

$$E = C \cdot e^{-\frac{\pi^2}{L^2} kt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

C denoting the value of the constant

$$A(1 + 2Bz_0) \frac{8}{\pi^2} \frac{Ih}{kq} L(1 - B_1).$$

The quantity L may, without prejudice to the accuracy of the experiments made according to this method, be taken very small, and thereby the value of the quantity $\frac{9\pi^2}{L^2} k$ be considerably raised. In the experiments discussed below, L amounted to 0.522 centim., and for k at the temperature of the apart-

ment a value of about 0.20 to 0.25 $\left[\frac{\text{centim.}^2}{\text{day}} \right]$ was found.

Under these conditions the term $\frac{1}{9} e^{-\frac{9\pi^2}{L^2} kt}$ becomes already after the lapse of an hour and a half so little (amounting then to only about $\frac{1}{12^{150}}$) that in comparison with the first term $e^{-\frac{\pi^2}{L^2} kt}$, it hardly need any longer be taken into consideration. In all the measurements made after this method the observation of the electromotive force began an hour and a half after the interruption of the current; accordingly equation (13) can be employed throughout as perfectly exact. The value of $\frac{\pi^4}{L^2} k$, being so considerable, causes the electromotive force to fall very rapidly as the time goes on (from one 10 seconds to another the diminution of the electromotive force was distinctly perceptible), and therefore renders a determination of the electromotive force by the compensation method impossible. Nothing remains now but to measure the electromotive force present at a given moment by the first deflection which it produces in a sensitive galvanometer in the circuit of which it is inserted during a half-vibration of the galvanometer-needle. The currents, always lasting 5 seconds only, generated by this electromotive force, never attained intensities exceeding

$\frac{1}{6000} \left[\frac{\text{mgr.}^{\frac{1}{2}} \text{mm.}^{\frac{1}{2}}}{\text{sec.}} \right]$ in absolute electromagnetic measure. The alterations, of the concentrations at the time existing at the electrodes, evoked by these feeble and transient currents turn out therefore so slight that they can be altogether neglected. The first deflection s evoked in a galvanometer by an electromotive force is proportional to that force; and when the sensitiveness of the galvanometer remains constant and the resistance of its circuit invariable, the electromotive forces present at certain moments of time are to one another as the first deflections which they impart at those moments to the galvanometer-needle. The connexion between the first deflection s , obtained on a galvanometer when the diffusion-vessel is inserted at the moment t , for the time of half a vibration, in the galvanometer-circuit, and the time t has therefore the form

$$s = C_1 e^{-\frac{\pi^2}{L^2} kt},$$

in which C_1 depends on the above-discussed constant C , the resistance of the galvanometer-circuit, and the quality of the galvanometer.

The quotients of all the pairs of consecutive first deflections

$s_0, s_1, s_2, \dots$ observed at the moments $t_0, t_1, t_2, \dots$ separated by the same time-interval Δt , must therefore be constant if Fick's elementary law is the expression of the reality.

Results of the Observations.—In order, first of all, to show to what nicety the observations can be carried by the second method, I will state in detail the whole of the observations which I have obtained in the first series made according to this method.

The space between the two zinc plates of the diffusion-vessel ($L=0.522$ centim.) was filled, on the 30th October, at ten o'clock, with a solution of zinc sulphate of the concentration 0.3120. After the filling, not the slightest trace of electromotive force appeared between the two zinc electrodes. Thereupon a galvanic current, maintained accurately constant, of the intensity 1.1 (mgr. $\frac{1}{2}$ mm. $\frac{1}{2}$ sec. $^{-1}$) in electromagnetic measure, was passed, from ten till twelve o'clock, in the direction from below upward, through the diffusion-vessels. From twelve to half-past one the diffusion-vessel was left untouched, at a constant temperature; the observations of the further course of the electromotive force between the two zinc electrodes commenced at about half-past one. Every two minutes the diffusion-vessel was inserted for the duration of a half-vibration of the needle (5.2 seconds) in the circuit of a sensitive galvanometer with a considerable damping; and the first deflection observed. The needle came to perfect rest in about $1\frac{1}{2}$ minute; so that this was the initial state of the needle at each observation. The resistance of the diffusion-vessel was vanishingly small compared with the resistance (366 m. u.) of the rest of the galvanometer-circuit.

The following Table gives, in the second and fifth columns, the first deflections s obtained (reduced to arcs); in the third and sixth columns stands the temperature of the diffusion-vessel, noted every ten minutes. The last column gives the differences of the ordinary logarithms of each two deflections which are distant half an hour from one another. The numbers of this last column show with sufficient distinctness the accuracy of observation that can be attained in this way, and to what degree the elementary law which forms the foundation of the theory corresponds to the facts. They, further, make it evident that the determination of the quantity of diffusion, k , can with tolerable exactness be effected from a few observations made during a fraction of an hour.

	$s =$	$T =$		$s =$	$T =$	$\Delta \log =$	
h m			h m				
12 0	170.3	18.4	12 30	141.9	18.4	0.07923	} 0.07870
2	168.3		32	140.3		0.07902	
4	166.4		34	138.7		0.07907	
6	164.5		36	137.1		0.07913	
8	162.5		38	135.5		0.07890	
12 10	160.5	18.4	12 40	133.9	18.3	0.07870	
12	158.5		42	132.3		0.07847	
14	156.6		44	430.7		0.07851	
16	154.6		46	129.0		0.07862	
18	152.7		48	127.4		0.07867	
12 20	150.9	18.4	12 50	126.0	18.3	0.07832	} 0.07874
22	149.0		52	124.5		0.07802	
24	147.3		54	123.0		0.07829	
26	145.5		56	121.5		0.07828	
28	143.6		58	119.9		0.07933	
1 0	118.4	18.3	1 30	98.8	18.4	0.07859	
2	117.1		32	97.7		0.07867	
4	115.8		34	96.5		0.07918	
6	114.5		36	95.4		0.07926	
8	113.1		38	94.3		0.07895	
1 10	111.8	18.3	1 40	93.4		0.07809	} 0.07874
12	110.4		42	92.1		0.07871	
14	109.1		44	90.9		0.07926	
16	107.7		46	89.8		0.07894	
18	106.5		48	89.0		0.07796	
1 20	105.2	18.4	1 50	87.8	18.5	0.07853	
22	103.8		52	86.7		0.07816	
24	102.6		54	35.5		0.07918	
26	101.3		56	84.4		0.07927	
28	100.0		58	83.5		0.07831	

We get from the general mean value, 0.07872, in consequence of the relation

$$0.07872 \times 2.303 = \left(\frac{\pi}{0.522} \right)^2 k \frac{1}{48},$$

the value

$$k = 0.2404 \left[\frac{\text{centim.}^2}{\text{day}} \right] \text{ for the temperature } 18^\circ.4.$$

Influence of Temperature upon the Quantity k of Diffusion.—From the observations made according to the first method of experiment, it is evident that the value of the quantity k of diffusion increases considerably with rising temperature. To establish this influence I have made twelve series of experiments, at three different constant temperatures, according to the second method, in exactly the same manner and with exactly the same range as those just now communicated in detail. The found course of the diffusion was as perfectly regular in *each* of these twelve series as in the first. I will, on this account, omit the long series of numbers, and give only the final results:—

Date.	k .	T.	
Nov. 1.	0.1244	1.1	Mean, $k=0.1252$ for $T=1^{\circ}.20$.
Nov. 2.	0.1245	1.0	
Dec. 6.	0.1262	1.3	
Dec. 7.	0.1257	1.4	
Nov. 3.	0.2427	18.8	Mean, $k=0.2421$ for $T=18^{\circ}.55$.
Nov. 4.	0.2443	18.9	
Dec. 8.	0.2413	18.5	
Dec. 10.	0.2400	18.0	
Nov. 7.	0.4128	44.4	Mean, $k=0.4146$ for $T=44^{\circ}.70$.
Nov. 8.	0.4163	45.2	
Dec. 11.	0.4101	44.1	
Dec. 12.	0.4194	45.1	

From these results it follows that the quantity of diffusion, k , within the temperature-interval 0° to 45° , increases in an approximately linear manner with the temperature; the relation

$$k=0.1187 [1+0.0557t]$$

corresponds very well with the observations. For the temperature $9^{\circ}.5$ we obtain from this relation the value $k=0.1815$; the series of experiments made at the same temperature according to the first method of experiment gave 0.1849.

Influence of the Degree of Concentration upon Diffusion-quantity k .—Fick's elementary law of hydrodiffusion includes the hypothesis that the diffusion-quantity k is independent of the degree of concentration.

A closer analysis of the experimental procedure made use of by us makes it evident that both methods of experiment are adapted to enable us to give a trustworthy decision for or against that hypothesis only in case the course of diffusion be studied in a series of solutions with as different initial concentrations as possible. If (as is to be expected, supposing that this dependence at all exists) the dependence of the quantity k upon the degree of concentration is but very slight, it can hardly be recognized in one and the same series of observations, even with the nicest reading, on account of the differences of concentration therein being too slight. In order to test the hypothesis in question, I repeatedly ascertained the quantity of diffusion for two solutions with the very different initial concentrations $z_0=0.214$ and $z_1=0.318$, at the temperature of the apartment, by the second method of experiment. The values found are contained in the two following Tables.

Solution with the Concentration 0·214.

	T.	k.
Nov. 14.	18·0	0·2399
Nov. 17.	19·0	0·2435
Nov. 18.	17·6	0·2397
Dec. 21.	18·8	0·2428
Dec. 23.	17·1	0·2384
Dec. 27.	16·9	0·2377
Mean	17·9	0·2403

Solution with the Concentration 0·318.

	T.	k.
Nov. 10.	18·1	0·2297
Nov. 11.	18·9	0·2331
Nov. 12.	18·1	0·2288
Dec. 20.	17·9	0·2306
Dec. 22.	17·3	0·2248
Dec. 24.	17·8	0·2264
Mean.....	18·0	0·2289

It hence follows that the diffusion-quantity k is not independent of the concentration, but diminishes, though very slowly, with ascending concentration.

In the theory of diffusion, therefore, Fick's elementary law requires correction in the same manner as Fourier's elementary law in the theory of heat-conduction: as there the quantity of the internal conduction slowly diminishes as the temperature rises, so here the quantity of the diffusion gradually sinks to smaller values as the concentration increases. Fick's hypothesis expresses the course of diffusion with only the same accuracy with which Fourier's elementary law represents the process of the conduction of heat in rigid substances.

Appendix.—Remarks on so-called Unpolarizable Electrodes.

Dubois-Reymond, in 1859, ascertained that the polarization of amalgamated zinc electrodes in aqueous solution of sulphate of zinc, on the employment of extremely feeble polarizing currents, is vanishingly little, at all events incomparably less than that of any other combination. He thought that the combination amalgamated zinc electrodes in zinc-sulphate solution might with reason be designated "unpolarizable."

These experiments have hitherto been misconceived, almost without exception, by all who have reported thereon. Although from Dubois-Reymond's statements it is evident that the

unpolarizability exists only *approximately* with extremely feeble polarizing currents, and entirely disappears as the intensity of the current is increased, since that time the false opinion has prevailed in the literature of galvanism that amalgamated zinc electrodes in zinc-sulphate solution are unpolarizable.

The second of the methods of diffusion employed by me exhibits in the most striking manner the considerable polarizability of the combination in question. Not only does a demonstrable polarizability exist, but upon it the most rigorous and most convenient method of measurement of the course of diffusion can be based. At the same time this method reveals, with all the clearness that can be wished for, the genesis of this polarization. The polarizing is not a consequence of the electrolysis that goes on at the electrodes (the separation of the gases), but the result of the alterations of the concentrations in the layers of the zinc-sulphate solution in contact with the electrodes, conditioned by the migration of the ions.

The correctness of these assertions I have already, about six years ago, in an altogether different way demonstrated, in the Berlin Laboratory, by the following simple experiment:—Two amalgamated zinc electrodes are placed in a solution of sulphate of zinc, not vertical and opposite to one another, but horizontal and one above the other. A polarizing current is passed during a short time through the combination, which is then inserted in the circuit of a sensitive galvanometer. In the first place, with all, even the weakest intensities of the polarizing current, and with any, even the shortest time during which the current passes through it, the combination constantly shows itself to be so polarized that the polarization-current generated possesses the opposite direction to that of the polarizing one. Further, *the quantity of polarization produced is very different according to the direction of the polarizing current*: if the polarizing current passes through the combination during a certain time in the direction from below upwards, the produced electromotive force of the polarization (measured by the galvanometer-deflection) proves to be *from 5 to 7 times as great* as in the case where the same polarizing current during the same time passes through the combination in the opposite direction. In the first case the alterations of concentration produced at the electrodes (at the lower an increase, at the upper a decrease) by the polarizing current are but extremely little diminished by the diffusion; in the latter case, on the contrary, the action of gravity almost completely destroys the alterations of concentration of the zinc-sulphate solution produced at the electrodes by the passage of the current through it.

LXII. *Experimental Researches on the Temperature of the Sun.*
 By F. ROSETTI, *Professor of Physics in the University of Padua.*

[Concluded from p. 449.]

VIII. *Thermal Radiation of the Sun.*

THE observations on the thermal effect of the radiation of the sun were made with pile No. 1 (Duboscq), placed upon a platform near to the galvanometer. The pile could easily be directed so that the rays of the sun fell perpendicularly on its face. Four readings of the galvanometer were made for each observation :—the first with an open circuit, in order to determine the point of rest on the scale ; the second with a closed circuit and the case of the pile closed ; the third with the case open and the rays admitted ; the fourth with the window of the case open, but intercepting the rays of the sun with a small circular screen with double walls placed at about a metre from the pile, so that its shadow was thrown on the window. In this fourth observation the face of the pile receives the radiation from the part of the sky surrounding the sun, which exercises an influence also in the third observation. Thus, by taking the difference between the third and fourth readings, the thermal effect of the solar radiation is obtained in scale-divisions. I should remark in passing, that the fourth reading was nearly always negative.

The electric current produced by the solar radiation was so great, that I had to introduce into the circuit sixteen Siemens's resistance units.

In order to determine the depth of the atmospheric layer traversed by the solar rays before arriving at the pile, in the first experiments the time of the third observation was noted : thus by means of the ephemerides the zenithal distance could be calculated. In the later experiments the determinations were made directly with a theodolite. When one of the observers had made the first two readings, the other (who was on the platform) opened the window of the case containing the pile, observing at the same time by means of a theodolite the height of the sun above the horizon ; in the meantime the first observer had made the third reading with the galvanometer. The hygrometric state of the air was also ascertained each time ; and in order to obtain an immediate indication, a hair hydrometer was used. This hygrometer had been graduated by comparing it with a condensation hygrometer. The following Table shows relative humidity corresponding to the

degrees marked on the instrument. When the air contained an excessive amount of moisture the index went beyond 100° .

TABLE IV.

Hygrometer.	Relative humidity.
60	0.361
65	0.405
70	0.450
75	0.500
80	0.550
85	0.596
90	0.646

Barometric observations were also made ; but we shall see that these indications, as well as those of the hygrometer, cannot be used except in cases of simultaneous observations made at different altitudes. Before giving the tables of observations, it may be useful to show how the value of ϵ , representing the thickness of the atmospheric stratum traversed by the rays of the sun, has been calculated. If at the time of the observation the sun is not more than 60° from the zenith, the formula $\epsilon = \sec Z$ gives us the value nearly enough. When the sun is lower, it is necessary to calculate ϵ by a formula which gives exact results. At the Mont Souris observatory Pouillet's formula is employed ; but when the distance from the zenith is great, this gives too small a value for ϵ . The values which I have entered in the following Tables are calculated from the formula of Bouguer and Laplace, which gives exact and concordant results.

In order to obtain the values quickly, I constructed a curve, the abscissæ of which represent the zenithal distances, and the ordinates the corresponding values of ϵ . In order to trace the curve I used the values contained in the following Table, which have been calculated by Forbes with the formula of Laplace.

TABLE V.

Distance from zenith. Z.	ϵ .	Distance from zenith. Z.	ϵ .
0	1.000	75	3.809
10	1.016	80	5.57
20	1.065	85	10.22
30	1.156	86	12.2
40	1.306	87	14.9
50	1.555	88	18.9
60	1.995	89	25.1
70	2.902	90	35.5

From amongst the number of observations made from the month of August till December, I shall only give those of 28th and 29th September, which were very favourable, because the sky kept clear and the air was tranquil and very transparent.

TABLE VI.

Observations on the Thermal Radiation of the Sun, 28th of September, 1877.

Time.	Distance from zenith. Z,	Deflections of galvanometer. y.	Hygrometer.
h m	° '	div.	
8 30	64 54	188.0	89
9 0	60 42	191.0	89
10 0	53 27	202.0	86
11 35	47 36	209.2	82
12 0	47 34	210.0	80
1 0	50 12	207.0	74
2 15	57 23	191.0	71
3 0	63 23	184.5	68
4 0	72 43	164.0	69
4 35	78 36	131.0	76

TABLE VII.

29th September. $H=768$, $t=16^{\circ}.5$ C.

Time.	Distance from zenith. Z.	Reading on galvanometer. y.	Hygrometer.
h m	° '	div.	
8 45	63 48	180.5	92.5
9 5	60 22	186.5	92.5
9 35	56 36	192.0	92.0
10 5	53 21	197.0	91.0
10 35	50 48	200.0	90.0
11 5	49 0	201.0	88.5
11 35	48 0	204.5	86.0
12 2	48 0	205.0	82.0
1 40	53 48	190.0	
2 5	56 36	189.0	74.0
3 5	64 30	174.0	74.0
3 45	70 36	155.0	71.0
4 30	78 5	130.0	74.0
4 50	81 24	109.0	74.0

Although the two days, 28th and 29th of September, were

very favourable for the experiments, we see, nevertheless, that at an equal zenithal distance (*i. e.* with an equal thickness of the atmospheric stratum for the rays to traverse) the observations of the morning are not altogether concordant with those of the afternoon. Amongst the observations of 29th of September we find, for example, that the observations of 9 h. 35 m. gave a deflection of 192 divisions, whilst that of 2 h. 5 m. only gave 189 divisions; and in both cases the distance from the zenith was equal to $56^{\circ} 36'$. In the same way, in the observations of 28th of September we obtain 191 divisions when the thickness of the stratum of air traversed, at 9 in the morning, corresponded to $60^{\circ} 42'$ distance from the zenith; whilst at 2 h. 15 m. we had the same deflection of 191 divisions, notwithstanding the decreased thickness of the atmospheric stratum. In general I found that the observations of the morning gave a somewhat smaller deflection than those of the afternoon, although the hygrometer almost always indicated a less degree of moisture in the afternoon than in the morning. But just this coincidence gives the key to the explanation of the phenomenon. In the morning the lower strata of the atmosphere, those which can influence the hygrometer, contain a large quantity of vapour, while the upper strata contain a much smaller quantity of it; but as the sun rises, the vapour also rises, and expands into the higher regions of the atmosphere. In the morning the upper strata of the atmosphere are less saturated with vapour, and consequently absorb less heat; in the afternoon, on the contrary, the absorption becomes greater on account of the preponderating action of the vapour expanded into the higher strata, whilst at the same time the lower strata are drier.

On constructing curves with the data contained in the two preceding Tables, taking for abscissæ the values of ϵ corresponding to the distances from the zenith entered in the second column, and for ordinates the values of the deflections of the galvanometer, we obtain for each day two dissimilar curves. I preferred to construct only the curve for the afternoon, as, the points lying usually in a straight line, it could be drawn easily and with great precision. In Table VIII. the values of the coordinates obtained from the curves are given.

TABLE VIII.

Date of observ.	ϵ .	Scale-divisions. y .	Date.	ϵ .	Scale-divisions. y .
Sept. 28.	1.30	217	Oct. 21.	4.80	137.2
"	1.40	213	"	5.40	128.3
"	1.50	210	Nov. 1.	1.80	197.3
"	1.60	205.3	"	2.00	187.5
"	1.80	198.0	"	2.50	170.2
"	2.00	191.2	"	3.00	156.0
"	2.20	185.0	"	3.50	145.0
"	2.60	172.3	"	4.00	136.7
"	3.00	162.0	"	4.50	130.0
"	3.20	158.0	"	5.00	123.6
"	3.60	152.0	"	2.60	166.1
"	4.00	145.0	"	3.00	160.2
"	4.97	131.0	"	3.20	153.0
Sept. 29.	1.40	206.8	"	3.50	145.5
"	1.50	202.9	"	4.00	138.0
"	2.00	184.8	"	4.50	132.7
"	2.50	169.0	"	4.84	130.0
Oct. 2.	1.60	198.7	"	5.00	128.1
"	2.00	178.5	"	5.50	123.0
"	2.50	164.9	"	6.69	109.0
"	3.00	155.0	"	1.80	197.4
"	3.30	158.0	Oct 19.	2.00	190.5
"	3.60	147.3	"	2.50	174.9
Oct. 3.	1.60	181.5	"	3.00	161.8
"	2.00	164.8	"	3.50	149.3
"	2.50	149.2	"	4.00	137.5
"	3.00	135.8	"	4.50	125.5
"	3.50	125.0	"	5.00	114.0
"	4.00	117.0	"	5.50	104.2
Oct. 10.	1.60	206.0	"	6.00	98.8
"	1.80	198.0	"	5.50	118.0
"	2.00	191.5	"	6.00	112.0
"	2.30	182.5	Dec. 17.	2.7	180.0
"	2.80	170.9	"	3.3	165.5
Oct. 19.	6.50	90.0	"	3.9	154.0
"	7.00	83.0	Dec. 22.	2.7	162.3
Oct. 21.	1.80	200.9	"	3.3	148.0*
"	2.40	179.8	"	3.9	137.6
"	3.00	166.6	"	4.5	128.8
"	3.60	156.1	"	5.1	120.3
"	4.20	146.1	"	5.7	112.5

IX. Determination of the Thermal Absorption of the Atmosphere.

The deflections y which have been observed on the galvanometer are produced by the pencil of solar rays which falls on the face of the pile, after having traversed a stratum of air more or less thick according to the altitude of the sun. In this passage the thermal effect of the rays is greatly lessened through absorption by the air, and especially by the vapour

* Hygrometer = 100.

contained therein. Before comparing the thermal effect of the rays of the sun with that of hot bodies placed at a short distance from the pile, it is necessary to find out what is the diminution produced by the atmospheric absorption in order to be able to calculate the thermal effect of the solar rays which fall on the face of the pile. To do this we first require to find out the law of absorption of the rays of the sun during their passage through the terrestrial atmosphere. It has been admitted by many physicists that the law is expressed by the formula

$$q = ab^{\epsilon};$$

q being the quantity of heat received in a unit of time by the unit of surface perpendicular to the direction of the rays in a place on the earth's surface,

a and b being two constants, and

ϵ a number which represents the thickness of the atmospheric stratum traversed by the rays, taking for unity the height of the atmosphere.

a represents the quantity of heat which in unit time would be received on the unit surface perpendicular to the direction of the rays at the limits of the atmosphere; and this is the quantity which it is desired to determine.

b is the proportion of the diminution of this quantity when the rays have traversed in the perpendicular direction the earth's atmosphere; and this is called the coefficient of absorption.

Violle attempted to determine the hygrometric state of the air; and he made simultaneous observations at different heights on Mont Blanc. He found that the phenomenon was pretty well represented by the formula

$$y = ap \frac{H + (Z - z)kf}{760} \epsilon;$$

in which a, p, k are constants;

Z , height of the atmospheric stratum beyond which the vapour appears to cease;

z , altitude of the place of observation;

f , mean temperature of the vapour contained in the strata of air between Z and z ;

ϵ , thickness of the stratum of air traversed.

This is certainly the best method of expressing and estimating the law of atmospheric absorption; but it requires simultaneous observations at different altitudes, and is not applicable in the case of observations made at one and the same station. I have consequently been obliged to have recourse to other methods. I tried at first to apply the formula $q = ab^{\epsilon}$ to the curves obtained in some days of observations; but I soon

found out that this single formula is not sufficient to represent the phenomenon for one entire day. I then tried to employ other empirical formulæ, and in particular the two more complex exponential formulæ

$$q = ab\gamma^{\frac{\epsilon}{\gamma+\epsilon}},$$

$$q = a + m\beta^{\epsilon} + n\gamma^{\epsilon},$$

$a, b, m, n, \alpha, \beta, \gamma$ being empirical constants; but not one of the formulæ employed is able to represent with sufficient precision the whole phenomenon.

In using the following three couples of values with the formula $q = ab\gamma^{\frac{\epsilon}{\gamma+\epsilon}}$,

$$\begin{aligned} \epsilon &= 1.5, & q &= 203.8, \\ \epsilon &= 2.25, & q &= 174.0, \\ \epsilon &= 3.00, & q &= 152.0, \end{aligned}$$

we obtain

$$\begin{aligned} \gamma &= 7.06180, \\ \log b &= 8.966537 - 10, \\ a &= 309.27. \end{aligned}$$

E.	q observed.	q calculated.	Difference.
1.5	203.8		
1.75	192.6	192.76	0.16
2.00	182.8	182.88	0.08
2.25	174.0		
2.50	166.0	165.98	-0.02
2.75	158.9	158.71	-0.17
3.00	152.1		
4 i	132.7	134.42	+1.72

In this case the formula answers pretty well.

By applying the same formula to the curve of 28th September for the interval $\epsilon = 1.4$ and $\epsilon = 2.6$, we obtain

$$a = 292.25.$$

In the curve of 1st November, interval $\epsilon = 2.0$ and $\epsilon = 3.2$, we have

$$a = 333.24;$$

For the 10th October,

$$a = 339.95.$$

The other curves would give results as much smaller as the interval taken for the calculation of the constants was greater.

21st October, interval $\epsilon = 1.8$ and $\epsilon = 10$,

$$a = 260.9.$$

21st October, interval $\epsilon = 1.8$ and $\epsilon = 6$,

$$a = 272.2.$$

But between the calculated values and the observed values the differences are too great, as the following Table shows:—

E.	q observed.	q calculated.	Difference.
1.8	200.9		
2.4	179.8	183.97	+4.17
3.0	166.6	169.41	+2.81
3.6	156.1	156.79	+0.61
3.9	151.1		
4.2	146.1	145.7	-0.32
4.8	137.2	136.1	-1.1
5.4	128.2	127.58	-0.62
6.0	120.0		

The great differences which the calculated values exhibit in proximity to $\epsilon = 1.8$ show that the calculated value of a which corresponds to $\epsilon = 0$ is much less than the true value. Consequently I have been obliged to abandon all hope of making use of a formula which is only empirical, and content myself with dividing the curve into a certain number of parts, and applying to each portion an equation of the form of $q = ab^{\epsilon}$, calculating each time the value of the constants a and b . Naturally we thus obtain for each day of observation a series of different values for a and another for b . It may be useful to give a Table of the results obtained by applying this method to the observations of several days.

Date.	Interval between ϵ .	Values obtained by formula.		Mean value of ϵ .
		a .	b .	
Sept. 28.....	{ 1.4 and 2.0	273.68	0.83533	1.7
	{ 2.0 " 2.6	270.49	0.84080	2.3
	{ 2.6 " 3.2	250.79	0.86557	2.9
	{ 3.2 " 5.0	227.71	0.89954	4.1
Sept. 29.....	{ 1.4 " 2.0	272.3	0.82173	1.7
	{ 2.0 " 2.6	263.75	0.8371	2.3
	{ 2.6 " 3.2	237.17	0.8721	2.9
	{ 3.2 " 4.4	210.24	0.9054	3.8
Oct. 3	{ 4.4 " 7.0	196.33	0.909	5.7
	{ 1.5 " 2.1	271.96	0.77931	1.8
	{ 2.1 " 2.7	240.92	0.8256	2.4
	{ 2.7 " 3.3	234.20	0.8342	3.0
Oct. 21	{ 3.3 " 3.9	206.55	0.8667	3.6
	{ 1.8 " 2.4	280.25	0.8312	2.1
	{ 2.4 " 3.0	243.9	0.8807	2.7
	{ 3.0 " 3.6	230.7	0.8972	3.3
Oct. 14	{ 3.6 " 4.2	232.2	0.8955	3.9
	{ 4.2 " 4.8	226.8	0.900	4.5
	{ 4.8 " 5.4	234.6	0.894	5.1
	{ 1.7 " 2.3	251.20	0.8212	2.0
Dec. 17	{ 2.3 " 2.9	233.08	0.8484	2.6
	{ 2.9 " 3.5	217.49	0.8689	3.2
	{ 3.5 " 7.74	222.25	0.8635	5.6
Dec. 22	{ 2.7 " 3.3	262.7	0.869	3.0
	{ 3.3 " 3.9	245.9	0.887	3.6
	{ 2.7 " 3.3	245.8	0.8575	3.0
	{ 3.3 " 3.9	220.9	0.8856	3.6
Dec. 17	{ 3.9 " 4.5	212.7	0.8943	4.2
	{ 4.5 " 5.1	213.2	0.8938	4.8
	{ 5.1 " 5.7	212.7	0.8943	5.4

Now let us notice the values which were obtained for a and b . The values of b in each day's observations were found to increase with the value of ϵ ; but on different days, for one and the same value of ϵ we have different values of b . These facts have been noticed by former observers.

As I have already said, b represents the ratio between the quantity of rays which arrive at the face of the pile at the place of observation, after having traversed perpendicularly the depth $\epsilon=1$ of the atmosphere, and the rays which it would receive if the atmosphere did not exist, *i. e.* if ϵ were equal to 0. It is obvious that if the hygrometric state of the air were different on the different days, the values of b obtained by means of the formula corresponding to one and the same value of ϵ would also be different.

It is in accordance with the laws of absorption that b should increase as ϵ increases. In short, the absorption of rays which pass through a transparent body is greater in the first strata than in the subsequent ones. The observations of 20th September, 24th October, 21st October, and 22nd December show that, beyond a certain thickness ϵ , the coefficient of absorption becomes constant.

Let us now consider the values of a . Each of these values represents, in the units chosen for the curve, the true quantity of heat which falls on the unit of surface at the limits of the atmosphere. We see, nevertheless, that as ϵ diminishes the value of a increases; and it is evident that, if we had been able to make experiments with less thicknesses of ϵ , we should have had greater values for a . If we express the values of a graphically, taking for abscissæ the arithmetic means of the values of ϵ (which are found in the last column of the preceding Table) which have been employed in the calculation of each of the values of a , and for ordinates the corresponding values of a , we find that the different points thus determined arrange themselves with very few exceptions in a straight line. The more favourable the condition of the atmosphere on the day of observation, the more marked is this regularity.

Consequently, since a more exact method was required, I decided to take as the true value of a that portion of the axis of the ordinates which lies between the origin and the point of contact of the above-mentioned straight line with the same axis. In order to obtain greater accuracy, I determined by the method of least squares the equation of the line $a=m+n\epsilon$, which approaches nearest to the points representing the values of a corresponding to the different mean values of ϵ ; and I took for the value of the quantity sought the number m . This calculation was made for several curves obtained under very different conditions, and gave for m such results as, if we take into consideration the causes which necessarily influence

observations of this nature, we ought to regard as pretty concordant. The following are the results:—

September	28.		$a = 317.17$
„	29.		$a = 328.66$
October	3.		$a = 329.72$
„	14.		$a = 306.96$
„	21.		$a = 298.96$
December	17.		$a = 346.70$
„	22.		$a = 354.45$
Mean			$a = 326.08$

The calculations made for four other days gave for mean value $a = 318.7$. Thus the mean of eight days was $a = 323.4$ divisions. In order to give an adequate idea of the exactness, I need only remark that, a being represented in divisions of the galvanometer when sixteen Siemens units were introduced into the circuit, one division would correspond to $\frac{1}{10}$ of a degree; so that the greatest difference between the values of a and the mean value is not more than 3 degrees.

I wished to find out what values b ought to have in order that the formula should give for a the value which we have just determined. I found that if we subtract 0.08 from each value of b calculated, we obtain a number, $b_1 = b - 0.08$, which gives satisfactory values for a . Thus, if with the numbers deduced from the curves (Table VIII.), we reckon the value of a by the formula $y = ab_1$, giving to b_1 the values found for b (Table IX.) minus 0.08, we have values which are not far from the mean, $a = 323.4$ (see the following Table).

28th September.				
E.	y .	b .	b_1 .	$a = \frac{y}{b_1 \epsilon}$.
1.4	213.0	0.835	0.755	315.68
2.0	191.2	0.840	0.76	331.03
2.6	172.3	0.865	0.785	323.23
3.2	158.0	0.900	0.82	298.16
29th September.				
1.4	206.8	0.822	0.742	314.04
2.0	184.8	0.837	0.757	322.49
2.6	166.1	0.872	0.792	304.57
3.2	153.0	0.905	0.825	283.16
4.4	132.7	0.909	0.829	308.59
21st October.				
1.8	200.9	0.831	0.751	328.46
2.4	179.8	0.881	0.801	306.25
3.0	166.6	0.897	0.867	305.49
3.6	156.1	0.897	0.816	323.15
4.2	146.1	0.896	0.820	343.21
4.8	137.2	0.900	0.814	355.67

Another proof of the rule which has just been indicated can be given. If from different curves we deduce the values of y which correspond to one and the same value of ϵ , we have, obviously, different numbers. As, for example, we have for $\epsilon=2\cdot7$:—

September	28.	.	.	.	.	.	174·0
„	29.	.	.	.	.	.	163·3
„	30.	.	.	.	.	.	164·0
October	2.	.	.	.	.	.	160·0
„	18.	.	.	.	.	.	175·0
„	19.	.	.	.	.	.	169·0
„	21.	.	.	.	.	.	172·2
„	10.	.	.	.	.	.	172·4
November	1.	.	.	.	.	.	164·0
December	10.	.	.	.	.	.	168·0
„	17.	.	.	.	.	.	180·0
„	22.	.	.	.	.	.	162·2

The arithmetic mean of these values is $y=168\cdot7$; and if we determine the mean value of b corresponding to $\epsilon=2\cdot7$, we obtain the number

$$b=0\cdot86.$$

By making $b_1=0\cdot86-0\cdot08=0\cdot78$, and by substituting the values of y , of b , and of ϵ in the formula $y=ab_1^\epsilon$, we obtain

$$a=330,$$

a value which is very near the mean 323. If, instead of $b_1=0\cdot78$, we had taken the values 0·75 and 0·80, we should have had respectively the values 308 and 367 for a , which shows that a very slight difference in the value of the coefficient of absorption b is sufficient to give very different values for a ; but that proves also that the method followed in determining the mean value of a is very appropriate, since the extreme values amongst those of which the mean has been taken are nearer to the latter than those which are obtained by varying the coefficient of absorption 2 or 3 hundredths.

We can therefore confidently assume that the value $a=323$ represents the solar constant expressed in scale-divisions of the galvanometer employed when there was a resistance of sixteen Siemens units in the circuit, in addition to that offered by the pile No. 1, the rheophores, and the wire of the galvanometer.

It is now only necessary to determine what the value of a signifies, in order to be able to compare the thermal effect produced by the radiation of the sun with that produced by the radiation from the artificial sources, and so determine in the end the effective temperature of the sun.

X. *Determination of the effective Temperature of the Sun.*

We have seen that the thermal effect which the solar radiation, falling perpendicularly on the blackened face of pile No. 1, would produce if there were no atmosphere, or if the pile were situated at the higher limit of the atmosphere, is expressed by

$$a = 323 \text{ divisions.}$$

The solar observations were made with sixteen Siemens resistance units introduced into the circuit, whilst those on the radiation from artificial sources were made with no other resistance in the circuit than that offered by the pile itself, the rheophores, and the wire of the galvanometer. In short, in order to compare the solar observations with the others and to apply to them the formula, it is necessary to convert the value of $a = 323$ into the value y , which would have given the same solar radiation if the sixteen Siemens units had been omitted. For this purpose it is necessary to find the resistance offered by the pile, the rheophores, and the wire of the galvanometer. The mean value from a dozen experiments was 3.408 Siemens units. The values furthest from the mean were 3.374 and 3.496. Nine experiments made another day gave a mean value of 3.411. I therefore took $R = 3.41$ Siemens units to express the total resistance. Separately, the resistances were:—

The wire of the galvanometer . . . $r = 1.522$

The rheophores $r_1 = 0.552$

The pile No. 1 $r_2 = 1.336$

The value of y can now be easily calculated :

$$y = \frac{R + 16}{R} a = \frac{19.41}{3.41} a = 5.6921 a.$$

Since $a = 323$,

$$y = 1838.5 \text{ divisions.}$$

The effective temperature of the sun may be defined as that temperature which an incandescent body of the same size placed at the same distance ought to have in order to produce the same thermal effect y if it had the maximum emissive power, *i. e.* $E = 1$. In this case we could apply the formula

$$y = mT^2(T - \theta) - n(T - \theta);$$

and if we consider the surrounding temperature during the observations to have been about 24° , giving $\theta = 297$, we obtain

$$T = 10238.4;$$

so that the effective temperature of the sun, represented in

degrees Centigrade, is

$$t = 9965^{\circ}\cdot 4,$$

if we only take into consideration the absorption produced by the terrestrial atmosphere. If we neglected this absorption we should have a lower temperature. In short, in the observations made, the maximum was obtained on September 28th at midday : this is represented by 210 scale-divisions, which gives for y the value

$$y = 5\cdot 6921 \times 210 = 1195\cdot 3.$$

If we introduce this value into the formula, we obtain

$$T = 8883\cdot 8,$$

giving

$$t = 8610\cdot 8.$$

This result will be greatly modified if we take into account the absorption exercised by the solar atmosphere. According to Secchi, the solar atmosphere exercises a very powerful absorption on the rays which proceed from the photosphere : on account of this absorption only $\frac{12}{100}$ of the solar radiation pass beyond the atmosphere of the sun, whilst $\frac{88}{100}$ are absorbed by it. If we regard this value given by Secchi as correct, we can calculate the thermal effect which the sun would produce if it were without atmosphere. This effect would be

$$y = 1838\cdot 5 \times \frac{100}{12} = 15320\cdot 8.$$

The formula gives

$$T = 20653\cdot 7,$$

and consequently

$$t = 20380\cdot 7.$$

There are still two causes which can modify these results ; but certainly their effect is slight, since their influences are contrary and so compensate one another. One of these causes is the value of the specific emissive power of the sun, which may possibly be less than unity ; and in that case the true temperature of the sun would be higher. The other cause is the transparency of the different strata of the solar atmosphere : although this is small, it is nevertheless certain that we receive the rays from several superposed strata ; and although their temperature is certainly lower than that of the photosphere situated underneath, nevertheless to the radiation of the latter a portion of the radiation of these strata joins itself ; and consequently in that case a lower temperature of the sun is sufficient to produce the heating measured by our instruments.

I think, then, that I may fairly conclude that the true temperature of the sun is not very different from its effective temperature, and that it is not much less than *ten thousand degrees* if we only consider the absorption of the terrestrial atmosphere, nor much more than *twenty thousand degrees* if we also take into consideration the absorption by the solar atmosphere, estimating the latter at $\frac{88}{100}$ of the total radiation of the sun.

LXIII. *On the Influence of the Solar Rays on Vegetation.*
By ROBERT HUNT, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS the first publication of the results obtained by me, in my investigations into the influence of the Solar Rays on Vegetation, was in the *Philosophical Magazine* for April 1840, I hope you will allow me to remove a wrong impression made by a paragraph in the Address of the President of the British Association at Sheffield, and explain the reason of my delay in doing so.

Dr. Allman stated that Messrs. Draper and Pfeffer had recently observed "that the solar spectrum is not equally effective in all its parts" in its influence on the growth of plants, thus leaving it to be inferred that this observation was new with them.

After the meeting (at which I was not present) I at once (August 25th) referred Dr. Allman to my papers on this subject. These are to be found in the *Philosophical Magazine*, as stated above, and in the Reports of the British Association for 1842, 1843, 1844, and 1846; all of which were summarized in my 'Researches on Light,' the first edition of which work was published in 1844. I was favoured with a reply from Dr. Allman on the 29th of August. After regretting that he had overlooked my investigations, he adds, "I need not say that I shall do all I can to correct the omission, and shall certainly take care that before the admission of the Address into the Transactions of the Association due justice shall be done to you in it."

With this I rested content, until I received a letter from Dr. Allman, dated November 9th, in which he writes, "I am truly sorry to say that the Transactions of the British Association have stolen a march on me, and come out without

giving me a chance of correcting my omission regarding your researches on the influence of light on vegetation."

I hope with this explanation of the cause of the delay, you will now allow me to assert my claim, especially as Dr. Allman, in a note of November 13th, says, "you are quite right in asserting your claim to priority" &c., and "you are liberty to make what use you please of my letter."

As a very early investigator into those most interesting phenomena, I cannot afford to allow my discoveries to be unrecognized, without an appeal. It is highly satisfactory to find this supported by so high an authority as Professor Allmann, who in his letter of the 9th November writes:—"I have been refreshing my memory of your researches, by reading your published account of them; and their completeness and conclusiveness render my regret the greater, that I had not given them the recognition they so well merit."

I am, Gentlemen,

Your obedient Servant,

ROBERT HUNT, F.R.S.

LXIV. *On an Analogy between the Conductivity for Heat and the Induction-Balance Effect of Copper-Tin Alloys.*
By W. CHANDLER ROBERTS, F.R.S.*

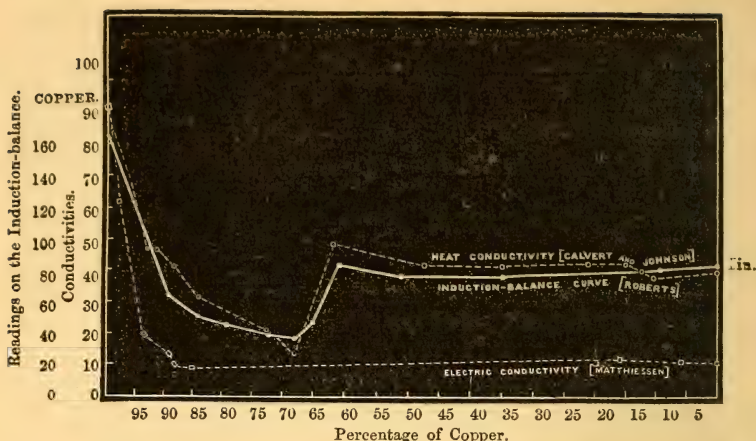
IN a paper submitted to the Society in June last, I pointed out that the results obtained when copper-tin alloys are examined by the aid of the induction-balance does not correspond with Matthiessen's determinations of the electric conductivities of the same alloys. The nature of the induction-curve remained therefore more or less obscure; but I have recently observed a remarkable resemblance between my own results† and those given by Calvert and Johnson‡ for the conductivity of heat, which had hitherto escaped me, as the authors did not plot their figures. The following diagram shows the relation of the curve published by me in July last with those of Calvert and Johnson and Matthiessen respectively, which I find for the first time placed in juxtaposition in a valuable report on the copper-tin alloys recently issued by the United-States Government§.

* Communicated by the Physical Society, having been read on November 8th.

† Phil. Mag. (5) vol. viii. p. 57, 1879.

‡ Phil. Trans. vol. cxlviii. 1858, p. 349.

§ Report made under the Direction of the Committee on Metallic Alloys. Washington, 1879.



Note.—The curves for the conductivity of heat and electricity are taken from Plate xiv. of the Report on Copper-Tin Alloys, above referred to. The latter curve should, however, be moved slightly to the left, as Matthiessen's numbers represent *volumes*, and not weights, per cent. of copper.

The earlier results, which were obtained by observations on blocks of alloy 18 millims. square and 7 millims. thick, have since been verified (except in the case of pure copper, which as originally given is too low) by a series of disks 24 millims. in diameter and 3 millims. thick, formed in the lathe, or, in the case of the very brittle ones, by filing.

The close agreement between the conductivity for heat and electricity has, as is well known, been pointed out by Wiedemann and Franz*; but as the similarity has hitherto, I believe, only been observed in isolated cases, the above results on an extended series of alloys may be of interest.

Calvert and Johnson claim a high degree of accuracy for their results; but they were subjected to adverse criticism, as Wiedemann† objected to the apparatus they adopted and to the small size of the bars employed. It is therefore the more important to point to the almost absolute identity of the two curves representing phenomena of which the manifestations are so different.

The respective values of the alloys were ascertained by placing the metal to be examined on one side of the balance and by superposing a graduated wedge-shaped scale of zinc over the opposing coil, as has already been described by Prof.

* Poggendorff's *Annalen*, vol. lxxxix. [1853], pp. 497-531.

† *Ibid.* vol. cviii. [1859], pp. 393-407.

Hughes*. The figures given, can however, only be considered to represent the general relation among themselves of the various alloys in the series, as the zinc scale does not give absolute values.

The induction-balance curve, as I have previously pointed out, bears an evident relation to the curve representing the density of the same alloys; but its divergence from Matthiessen's curve† of conductivity is singular. This may perhaps be explained by the fact that Matthiessen does not appear to have examined any alloy between those which contain respectively 16.4 and 85.1 volumes per cent. of copper, probably because the alloys between these points are too brittle to permit their being formed into wire. It may be noted, however, that the curve of the tin-gold alloys, which belong to the same group as the copper-tin, is given in his well-known paper‡ in 1860, and does not show an unbroken line in the horizontal portion. Certain intermediate alloys, in the form of rods, are now being examined by Dr. Lodge, whose results will doubtless clear up the point.

The alloys which occupy the critical points of the induction-balance curve are very interesting. They may be represented by the formulæ SnCu_3 and SnCu_4 respectively; and although they only vary by a single equivalent, or by 6.49 per cent. of copper, their structure and appearance differ widely. The latter, SnCu_4 , is a speculum-metal. It has a large conchoidal fracture and a yellow-grey tint. SnCu_3 , on the other hand, has a blue-grey colour and a coarse surface of interrupted crystalline planes. By successive additions of copper, this alloy seems to pass into the other without any sharp break.

Possibly both alloys are chemical combinations; and the difference of their ordinates probably marks a different allotropic state. For further information on such questions, however, we may look with confidence to Prof. Hughes's beautiful and simple instrument, which will also help us to determine whether the relation between conductivity for heat and electricity is really as exact as it has hitherto been supposed to be.

* Phil. Mag. (5) vol. viii. p. 50.

† Brit. Assoc. Report, 1863, p. 37, and Chem. Soc. Journ. 1867, p. 212.

‡ Phil. Trans. vol. cl. p. 161.

LXV. *Note on a Determination of the Specific Electrical Resistance of certain Copper-Tin Alloys.* By OLIVER J. LODGE, D.Sc., Assistant Professor of Physics in University College, London*.

ON the 5th of November I received from Mr. Chandler Roberts three rods of certain very brittle copper-tin alloys which he had with some difficulty cast, in order that I might determine their specific resistance per unit of volume by ordinary processes, because the readings which the induction-balance gave for disks of these three metals indicated that their conductivities did not by any means agree with Matthiessen's curve of electric conductivity for copper-tin alloys, though, on the other hand, they did agree with some severely criticised and discarded experiments of Messrs. Calvert and Johnson on the conductivities of these same alloys for *heat*.

The rods, which are hereafter spoken of as A, B, and C, were about 9 millimetres thick and from 30 to 40 centimetres long. The method which I employed to determine their conductivity was scarcely altered from that which was described by Prof. Foster and myself in this Journal in 1875†; and the results which I have obtained I believe to be somewhat accurate. It was not worth while to aim at any very *excessive* accuracy, though the method is quite capable of it (by a calibration of the bridge-wire), because the temperature of the alloys was a little indefinite, being simply that of the room.

The three rods were laid end to end in a groove in a long piece of wood so that they just projected above its surface; and their ends were then screwed up into secure contact with pads of tinfoil between. The resistance of the contacts is not of the slightest consequence, provided it remains constant and is moderately small, all measurements being made within the length of the rod itself.

A bridge-slider was arranged so as to make contact with one of the rods at a point whose position could be read by a millimetre-scale fixed at the proper level. The length of the portion of the rod examined could be ascertained within the tenth of a millimetre; and the resistance of this portion could also be read with great accuracy. The least satisfactory part of the measurement is the gauging of the diameter of the rod; for they are not perfectly cylindrical, and the *average* cross section has to be taken. I would have had the rods turned; only two of them, viz. B and C, are far too brittle to be touched with any tool.

* Communicated by the Physical Society.

† Foster and Lodge, "On the Flow of Electricity in a Plane.—Part II.," Phil. Mag. December 1875.

The observation of resistance consisted in placing the slider at a definite point on one of the rods, and feeling about on the bridge-wire with another slider for a point of the same potential. The slider on the rod was then shifted to another part of the same rod and the bridge-slider shifted correspondingly. The shifts on the bridge-wire (German silver) were of course rather small; but they could be read to the tenth of a millimetre; and, moreover, the resistances in the circuit were so adjusted that any step along the rods corresponded to a step of 3·62 times the resistance along the bridge-wire. It is needless to say that a weak battery and a reflecting galvanometer were used, and that the contacts were only made momentarily. As an example of the error to be expected I may quote the following.

Two different steps along rod A gave, as the step on the bridge-wire corresponding to the resistance of 1 centimetre length of the rod A, the numbers

·0328 and ·0325 centim.

Two different steps along rod B gave

·0269 and ·0270 centim.

Different steps along rod C, which was a trifle less uniform, gave

·123, ·121, ·1177, ·1173, ·1185, ·1171 centim.

As the step ·1185 was deduced from the entire length of the rod, I have taken this as the mean. The steps which gave ·123 and ·121 were short ones; and their probable error is therefore greater than that of the others. Moreover they were made at the thinner end of the not quite cylindrical rod. As regards the mean cross section of the rods, it was estimated by taking a number of readings of the diameters in all parts of the rods by means of a micrometer-gauge reading to the hundredth of a millimetre.

The mean diameter of rod A was ·879 centim.

"	"	"	B	"	·873	"
"	"	"	C	"	·869	"

The rods had been cast in the same mould; but C had contracted more than the others.

On the 7th of November Mr. Roberts sent me three more rods, which I labelled D, E, F, and then determined their resistance in the same way.

The mean diameter of these rods, which were perfectly strong and might have been drawn into wire, was

D	.	.	.	.	.	·880 centim.
E	.	.	.	.	.	·862 "
F	.	.	.	.	.	·872 "

These had had their surfaces filed after casting.

To get the value of the bridge-wire-readings in absolute measure, a half-ohm coil of German-silver wire at the temperature of the room (*i. e.* of the bridge-wire) was inserted in the place of the rods, and readings taken of the steps corresponding to it. The scale of magnification was of course again observed. Instead of being about 3·6, it was now about $\frac{1}{3}$.

The result of the whole series of measurements may be summed up in the following Table:—

Specific Resistances of the six Copper-Tin Alloys
in square centimetres per second.

A	12960
B	10960
C	47660
D	11830
E	17090
F	15270

I consider these accurate to the second figure, the third figure is probably affected by experimental errors.

If we choose to express the results in terms of conductivity, we have the following:—

Conductivity of a Centimetre Cube of the six Alloys
in B.A. units.

A	77100
B	91200
C	21000
D	84500
E	58500
F	65500

The specific resistance of the alloy C is thus four times that of B or D.

The composition of the alloys by weight, Mr. Roberts informs me, is as follows:—

A	19·2 per cent. of copper	80·8 tin.
B	61·8 " "	38·2 "
C	68·3 " "	31·7 "
D	0·0 " "	100·0 "
E	87·4 " "	12·6 "
F	90·3 " "	9·7 "

Some of these alloys were examined by Matthiessen, viz. A, D, E, and F; and the conductivity of a wire 1 metre long and 1 millimetre thick is stated by him to be, in terms of the B.A. unit,

of the metal A	6·0
" D (pure tin) . . .	5·9
" E	4·2
" F	5·9

To reduce these numbers from a wire to a centimetre cube we must multiply them by $\frac{40000}{\pi}$, when they become

for A		76400
„ D		75100
„ E		53500
„ F		75100

of which, therefore, A and E agree tolerably with my measurements on the rods.

Matthiessen also measured the conductivity of pure copper, his number when reduced being

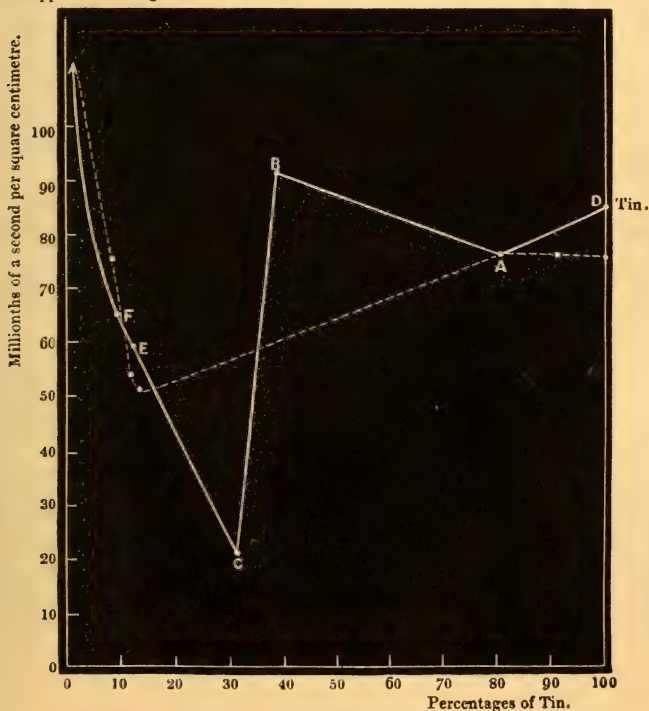
Copper 610000

or about eight times that of pure tin.

The results may be plotted in order to compare them more readily with the readings given by the induction-balance. The abscissæ represent percentages of tin ; the ordinates represent conductivities divided by 1000.

Specific Conductivities of Copper-Tin Alloys.

Copper is at a height 610.



The continuous broken line joins together my six points of observation. The dotted line is Matthiessen's curve of conductivity on the same scale, viz. absolute measure, with some of his points of observation marked on it; but no alloys between somewhere about E and A seem to have been actually examined by him.

The position of pure copper (from Matthiessen's measurement) cannot be shown in the diagram: the conductivity decreases at such a tremendous rate with the slightest percentage of tin that the ordinate for pure copper is 600 scale-divisions, whereas the greatest height shown in the diagram, that of the point B, is only about 90.

It is unnecessary to call attention to the extraordinary behaviour of the alloy C containing 31·7 per cent. of tin (that is, the alloy SnCu_4), or to point out how easily it might have been supposed to be a distinct metal.

LXVI. *On a Simple Method of using an insignificant Fraction of the main Current produced by a Dynamo-electric Machine, for Telegraph-purposes.* By LOUIS SCHWENDLER*.

THE currents which a dynamo-electric machine is capable of generating through a small external resistance are enormously strong, sufficiently constant, and exceedingly cheap; I always thought, therefore, it would be of technical as well as of economical importance to use them for signalling-purposes.

The only difficulty was how to solve the problem practically. Manifestly the currents could not be produced through the telegraph-lines, in the ordinary manner of applying dynamo-electric machines; for, in the first place, telegraph-lines offer high resistances, and, in the second place, the use of *the closed-circuit system* would become imperative. However, some time ago a very simple method occurred to me, which appears to contain the germs of practical success; and having made of late some experiments on the subject, I do not hesitate to communicate the result.

Suppose we have a dynamo-electric machine, the two terminals of which are connected by a resistance r through which any kind of *useful work* is to be performed by the current. For instance, during night r may consist of an electric arc, and the useful work done by the current is given out as *light* for the *signalling-office*; or during daytime r may consist of another

* Communicated by the Author.

dynamo-electric machine which acts as an ordinary electromagnetic engine, performing some useful mechanical work, *i. e.* pulling the punkhas, lifting messages, producing a draught of cool air, &c.; or the current is made to pass through a galvanoplastic apparatus in connexion perhaps with the surveyor-general's office, &c. Now connecting the *negative pole** of such a dynamo-electric machine to earth, the positive pole to *all* the lines terminating in a telegraph-office, while the two poles are permanently connected by the resistance *r* through which the current produces the useful work above mentioned, then it will be clear, without demonstration, that *all* the lines so connected can be provided with signalling-currents (which are exceedingly weak as compared with the main current) by simply *tapping* the main current, and without perceptibly reducing it, *i. e.* without influencing the *useful work* performed by the main current through *r*.

Supposing that the useful work performed by the main current repays all the expense connected with the erection and working of the dynamo-electric machine, then obviously this would be a method which supplied the signalling-current for *nothing*. This might be an inducement for telegraph-administrations to introduce the electric light, since they would get the signalling-currents into the bargain, and the costly and cumbersome galvanic apparatus might be dispensed with.

An example will show this more clearly. For instance, a Siemens dynamo-electric machine of medium size can easily be made to produce through an electric arc a current of 30,000 milli-oersteds, while not more than 3 milli-oersteds are required to work the Siemens's polarized relay with engineering safety. Supposing that the sent current is made equal to twice the current which is required to arrive, we have the following calculation for Calcutta office:—14 long lines terminate at Calcutta; hence $14 \times 6 = 84$ milli-oersteds would (as a maximum) have to be tapped off from the main current of 30,000 milli-oersteds. This represents a loss of only 0.28 per cent., which is so little that even the most sensitive eye would be unable to detect any variation in the light. Hence in this case we should feed the telegraph-lines with currents which actually cost nothing, as the electric light alone would repay all expense.

During my recent light experiments in London, it was experimentally established that the current in milli-oersteds which a dynamo-electric machine is able to produce can be

* In India we use positive signalling-currents.

expressed as follows :—

$$C = E \left\{ \frac{1 - e^{-K \left(\frac{v}{r+m} \right)^2}}{r+m} \right\} \times 1000.$$

E and K are two *constants* for any dynamo-electric machine;
E is an electromotive force in volts;

K is of such dimension that $v\sqrt{K}$ represents an electrical resistance;

m is the internal resistance of the dynamo-electric machine;

r is the external resistance through which the *useful work* by the main current has to be performed;

m and r are to be expressed in ohms.

The resistance of the leading wires has been supposed *nil*.

If we call R the resistance of a telegraph-line which we wish to feed from the main current, then the signalling-current passing into that line when the main current is tapped will be

$$\frac{Cr}{R+r} = E \left\{ \frac{1 - e^{-K \left(\frac{v}{r+m} \right)^2}}{r+m} \right\} \times \frac{1000r}{R+r};$$

and this current, in the case of the Indian lines, should not be less than 6 milli-oersteds. Hence we have the following equation :—

$$E \left\{ \frac{1 - e^{-K \left(\frac{v}{r+m} \right)^2}}{r+m} \right\} \times \frac{1000r}{R+r} = 6;$$

from which r can be calculated, since E, K, m , v , and R are known.

I need scarcely point out that, as R is invariably so large that r can be neglected in comparison with it; the current in *one* line only depends on the resistance of that line, and not on the resistance of the other lines in connexion with the dynamo-electric machine. Hence the signalling through one line is not influenced by the signalling on other lines; and in this respect the method is on a par with signalling through different lines by separate batteries.

We will take a special case:—For a Siemens's medium machine, making $r=3$, we have a main current of about 17,710 milli-oersteds; and the current passing into a line of 8000 resistance (800 miles of $5\frac{1}{2}$ wire) would be 6.6 milli-oersteds. Supposing that all the fourteen lines at Calcutta office are to be supplied with 6.6 milli-oersteds each, the cur-

rent carried off would be $6.6 \times 14 = 92.4$ milli-oersteds, or 0.5 per cent. of the main current.

It is best to make all the lines equal in resistance, by adding to the shorter lines some artificial resistance. This measure would prevent a dead earth (occurring on one of the lines and close to Calcutta) from having any effect on the working of the other lines.

In Europe, where the lines are much shorter, the signalling-currents supplied by a given dynamo-electric machine, working through a given resistance r , could be even much greater than 6.6 milli-oersteds.

For any given R (resistance of the line) the current can be increased by selecting a dynamo-electric machine with the right internal resistance.

The advantages of the method appeared to me sufficiently great to justify a practical trial:—

Experiment, October 11, 1879.—With a Siemens's dynamo-electric machine (medium size) I produced a powerful electric light; and between the poles of the dynamo-electric machine I connected up four artificial lines, each of 10,000 units resistance, with relays ranging between 500 and 1000 units. These four parallel circuits worked very well, singly and simultaneously. No variation of the electric light during telegraphing could be noticed, even when the line resistance was reduced to 1000 units. Further, the resistance of one line was increased to 20,000; and the signalling-currents were still sufficiently strong (1.6 milli-oersted).

Experiment, October 14, 1879.—Same as above; but a branch circuit was conveyed by the storeyard line (from the storeyard where the dynamo-electric machine with its electric light was put up) to Calcutta signalling-office (4 miles), and one of the Agra lines (850 miles in length) worked by this current.

The sent current at Calcutta was 9.6 milli-oersteds, the received current at Agra 1.85. The great loss was due to the exceedingly low insulation of the line near Calcutta. It is now the breaking-up of the monsoons, when the climate in Lower Bengal represents almost a hot vapour-bath.

Several messages were sent to Agra, but no variation in the electric light could be observed.

Calcutta, October 17, 1879.

LXVII. *Notices respecting New Books.*

- I. *Report on the Administration of the Meteorological Department of the Government of India in 1877-78.* Government Central Press.
- II. *Report on the Meteorology of India in 1877.* By JOHN ELIOT, M.A., *Officiating Meteorological Reporter to the Government of India. Third year.* Calcutta, Office of the Superintendent of Government Printing, 1879.
- III. *Report on the Madras Cyclone of May 1877.* By JOHN ELIOT, M.A., *Meteorological Reporter to the Government of Bengal.* Calcutta, Office of the Superintendent of Government Printing, 1879.

FROM the three works specified above we learn that the comprehensive system of meteorological observation set on foot about four years ago by the Government of India is in a satisfactory state. The system embraces one hundred and eleven observatories scattered over an area of $28^{\circ} 9'$ of latitude and $46^{\circ} 51'$ of longitude, ranging in vertical height to the extent of 11,532 feet, viz. from 6 feet above mean sea-level at Saugor Island, in latitude $21^{\circ} 39'$, longitude $88^{\circ} 5'$, to 11,538 feet also above mean sea-level at Leh, in latitude $34^{\circ} 10'$, longitude $77^{\circ} 42'$.

It is not improbable that the careful meteorological researches in India may throw some light on the question of sun-spot frequency and rainfall. Mr. Eliot in introducing his report has the following pertinent remarks :—"There can be no doubt that any periodicity in the great cause of terrestrial atmospheric changes—solar radiation—will tend to impress a corresponding periodicity upon the elements of terrestrial meteorological observation. But so far as can be judged from the observation of the magnitude of the sun-spots, the cyclical variation of the magnitude of the sun's face free from spots is very small compared with the surface itself; and consequently, according to mathematical principles, the effect on the elements of meteorological observations for the whole earth ought also to be small and proportional to the amount of variation of solar radiation. That this is probably the case is indicated by a statement given in Mr. Blanford's report of last year, that Mr. Meldrum's last and most careful researches show that the total cyclical variation of the rainfall does not exceed 15 per cent. upon the whole. This estimate of the amount of the variation would very probably be considerably reduced if it were possible to take into account the rainfall over the oceanic as well as the land area; for theory suggests that excess in the one would probably be accompanied by deficiency in the other. The known cyclical variation of the number of sun-spots, and therefore of the amount of solar radiation, can therefore at the best only account directly for a small and residual portion of the actual irregularities of rainfall &c. observed in a large area like India. It is however probable that at one part of the sun-spot period one effect of the variation of solar radiation may be

to exaggerate local irregularity (*i. e.* to give larger oscillations about the mean than usual); and it is also possible that in consequence of local, geographical, or other peculiarities, these exaggerated local irregularities may tend in certain cases to recur at the same parts of the earth's surface. So far as I have been able to investigate the evidence for and against the connexion between the sun-spot period and rainfall and other phenomena, it has confirmed this conclusion or hypothesis."

The Report on the Madras Cyclone of May 1877 is a most masterly production, entering fully as it does into the meteorology of India and the Bay of Bengal during the transition-periods preceding and succeeding the south-west monsoon, also the meteorology of India and the Bay of Bengal immediately antecedent to the generation of the cyclone, the meteorological phenomena accompanying the formation, advance, and dissipation of the cyclone, the probabilities of the occurrence of cyclones in the Bay at certain seasons, including an investigation into the causes of cyclone generation and motion in the Bay of Bengal.

The following are a few of the conclusions at which the author has arrived respecting cyclone generation, motion, and occurrence:—

"Cyclonic disturbances of small intensity are of frequent occurrence during the prevalence of the south-west monsoon or rainy season. Cyclones or revolving storms of great extent and intensity occur only at the transition-periods, *viz.* at the April transition-period preceding the south-west monsoon, and the October transition-period succeeding the south-west monsoon and ushering in the north-east monsoon."

"Cyclone occurrence is most probable during the October transition-period, when the most intense and extensive cyclones occur."

"The most powerful cyclones in the Bay of Bengal tend to occur at or about the minimum sun-spot period."

"Cyclones formed at the commencement of the April transition-period or near the end of the October transition-period are usually generated in the south of the Bay, and proceed towards the Coromandel coast."

"Cyclones generated during the latter part of the April transition-period or the greater part of the October transition-period (*i. e.* to the end of October) usually take their origin in the centre of the Bay to the west or north-west of the Andamans, and advance towards the coast of Ganjam, Orissa, or Bengal."

"There is a strong antecedent probability, derived from past experience, of the occurrence of at least one intense cyclone at each minimum sun-spot period."

"The invariable antecedents of powerful cyclones at the two transition-periods are approximate uniformity of pressure over and round the coast of the Bay of Bengal, light and variable winds or calms over a considerable portion of the Bay, and little or no rainfall over the coast region, as well as in the Bay: the weather is hence sultry, the sea smooth; the amount of aqueous vapour accumulates and finally gives rise to peculiar sky effects. The imme-

diate antecedent is heavy rainfall concentrated over a portion of the Bay, accompanied by strong indraught, most marked from the Indian Ocean at the entrance of the Bay. This indraught from the Indian Ocean gives rise to strong winds and heavy rain at the stations on the south and west coasts of Ceylon."

"The air-motion in cyclones is one of indraught; and therefore the wind-direction at any point is not at right angles to the direction of the centre. The stream-lines or the lines of air-motion are spiral curves. The relation between the wind-direction and the direction of the centre is probably not invariable, but depends on the intensity of the storm or the baric gradient. The law as laid down by Mr. Wilson in the report of the Midnapore cyclone is probably the nearest approximation. With the face to the wind, the direction of the centre is from ten to eleven points to the right-hand side."

LXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 421.]

June 11, 1879.—Prof. Joseph Prestwich, M.A., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. "On a Mammaliferous Deposit at Barrington, near Cambridge." By the Rev. O. Fisher, M.A., F.G.S.

2. "Further Discoveries in the Cresswell Caves." By Prof. Boyd Dawkins, M.A., F.R.S., F.G.S., and the Rev. J. M. Mello, M.A., F.G.S., with notes on the Mammalia by the former.

3. "On the Pre-Cambrian Rocks of Shropshire." Part 1. By C. Callaway, Esq., D.Sc. Lond., F.G.S.

The author commenced by describing the physical geography of the ridges intervening between the vicinity of Wellington and the Longmynd, viz. Lilleshall Hill, the Wrekin, and the chain of the Caradoc Hills. He passed on to describe the stratigraphy. At Lilleshall are slaty beds dipping about N.N.W., and a rhyolitic agglomerate; at the N. end of the Wrekin a granitoid soil, probably of clastic origin, with a mass of decomposed intrusive rhyolite. The Wrekin consists of rhyolitic agglomerates, with (probably) lavafloes and a few basalt dykes, the general dip of the bedded rocks being to the N. At the southern end (Primrose Hill) granitoid gneisses (some closely resembling the hornblendic gneiss of Malvern) occur. In the Caradoc range intrusive greenstones are more largely developed; but here also are bedded Pre-Cambrian rocks. Other smaller exposures in this vicinity were also described. The prevalent strike over the whole district is about E.N.E.—W.S.W. The evidence of their age is often very clear, as they are overlain (with marked unconformity) by quartzites (sometimes containing rhyolitic fragments) which are clearly much older than the Hollybush Sandstones.

4. "On the Occurrence of a Remarkable and apparently New Mineral in the Rocks of Inverness-shire." By William Jolly, Esq., F.R.S.E. &c., H.M. Inspector of Schools, and J. Macdonald-Cameron, Fel. Inst. Chem., F.C.S., &c.

In this paper the authors refer to a blue mineral of a somewhat remarkable character, which was specially noticed during an excursion of the Inverness Field Club in September 1877. This excursion was made to Englishton Moor and neighbourhood, distant westwards from Inverness about 5 miles, where the mineral occurs in scattered blocks. It has since been noticed at Moniack Burn, Reelig Glen, and South Clunes Farm, all in the same direction, but distant from Inverness about 10 miles; also near Dochfour House, at the north end of Loch Ness, close by Dochgarroch Lock of the Caledonian Canal. In colour and general appearance this mineral resembles crocidolite; but analyses point to its being more nearly related to ægirite, a member of the amphibole group, which has the general formula $\text{Si } 3(\frac{1}{2}\text{K} + \text{R}^3)$.

The mean of several analyses shows it to have the composition $6\text{SiO}_2, \text{Fe}_2\text{O}_3, 2\text{MgO}$.

June 25, 1879.—Prof. P. Martin Duncan, M.B., F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. "On the Evidence that certain Species of *Ichthyosaurus* were Viviparous." By Prof. H. G. Seeley, F.R.S., F.G.S.

2. "On *Rhamphocephalus Prestwichi*, Seeley, an Ornithosaurian from the Stonesfield Slate of Kineton." By Prof. H. G. Seeley, F.R.S., F.G.S.

3. "A Contribution to South-American Geology." By George Attwood, Esq., F.G.S.

The paper describes a line of country in Spanish Guayana, Venezuela, S. A., commencing from a small town called "the Port of Las Tablas," on the Orinoco river, extending about 150 miles, and consisting of a series of crystalline and altered rocks. Syenite is the first rock met with; and then are found granite, quartz-diorite, hæmatite, and magnetic iron-ores, gneiss, slaty rocks, gabbro, and diabase.

In the diabase the quartz-veins are found to contain large quantities of gold mixed with the vein-matter; the alluvial soil in the neighbourhood of the quartz-veins also contains gold nuggets and small grains of gold. Although quartz-veins are found in great numbers from the river to the interior, none of them has so far been found to contain gold in any appreciable quantity until the diabase is met with.

All the rocks analyzed show a higher percentage of silica than is generally found in other localities. Three analyses, made from one piece of diabase showing two distinct lines of alteration by weathering (on the original rock), prove that silica is readily dissolved under atmospheric influences, whilst alumina is not. Iron

oxides contain more oxygen near the surface than below it. Lime and magnesia are both readily soluble, but lime much more so than magnesia. Soda is more sensitive to weathering than potash. The rocks contain *more combined as well as uncombined* water on their surface than when sheltered from atmospheric influences.

The paper was accompanied by an appendix on the microscopical structure of some of the varieties of rocks by Prof. Bonney.

4. "On the so-called Midford Sands." By James Buckman, Esq., F.L.S., F.G.S.

The author quotes from the works of Professor Phillips and other authors certain passages, in which the sands below the Oolitic Limestones of the Cotteswold Hills and Dorsetshire are correlated with one another, and the name of "Midford Sands" is applied to the formation represented by these strata.

In opposition to these authors' views, Prof. Buckman maintains that two distinct Ammonite bands have been by them confounded with one another, that the sandy beds in the Cotteswolds really belong to a much lower horizon than do the similar strata in Dorsetshire and Somersetshire, and that, while the former lie quite at the base of the Inferior Oolite series, the latter represent a great part of that formation.

In support of this view Prof. Buckman points to the fact that a representative of the true Cephalopoda-bed lies at the base of the so-called Midford Sands of Somersetshire; he illustrates the rapid transitions which take place between sandy and calcareous strata in this part of the series; and in conclusion he shows, by the study of the somewhat fragmentary fossils found in the sands of Dorsetshire and Somersetshire, that they are the true equivalents of several different divisions of the Oolites of the Cotteswold Hills. He admits, however, that some Liassic forms range upwards into these beds.

5. "On the Physical Geography of the North-east of England in Permian and Triassic Times." By E. Wilson, Esq., F.G.S.

In this paper the author seeks to utilize the information he has acquired from the study of the Permian and Triassic rocks of the above district, towards solving some of the difficult and much-debated questions as to their origin. With this end in view he traces the various members of the Magnesian Limestone formation between Notts and Northumberland, noticing in particular the amplification of that group of rocks in northerly and easterly directions. Incidentally attention is called to the increased importance of the Marl Slates as a distinct and characteristic series. One of the main objects of the paper is to establish the pre-Permian origin of the Penine Chain. The nature and relative values of the stratigraphical breaks which, in the district in question, occur between the Carboniferous and Permian, the Permian and Bunter, and the Bunter and Keuper formations are severally dealt with. The author concludes by speculating as to the general conditions under which the Permians may have been formed, and the physical fluctuations

that may possibly have brought about the succession in one geological epoch of rocks so distinct in mineral constitution and in fossil contents as the Marl Slates, Magnesian Limestone, and Permian Marls.

6. "The Formation of Rock-basins." By J. D. Kendall, Esq., C.E., F.G.S.

The author discusses the mechanical difficulties involved in the glacier-excavation theory of lake-basins, and suggests that they are due to the action of falling water engulfed in the crevasses of the ancient glaciers, being thus of the nature of "giants' kettles," though on an enlarged scale.

7. "On the Diorites of the Warwickshire Coal-field." By S. Allport, Esq., F.G.S.

The diorites are intrusive in the lower and unproductive measures of the above field and the underlying Millstone Grit below Atherstone and Marston Jabet (two miles south of Nuneaton). The author describes their microscopic characters. One variety is very finely crystalline, and contains brown hornblende; another contains plagioclase felspar with a little orthoclase, small crystals of brown hornblende, many crystals of clear yellowish augite, and several pseudomorphs after olivine, with apatite, magnetite, &c. A mineral also occurs belonging to the hexagonal system, which the author suspects to be magnetite. Other varieties are described, one of which contains augite with hornblende. These rocks differ considerably from the syenites of Leicestershire.

8. "On *Lepidodiscus Lebouri*, a new Species of Agelacrinitidæ, from the Carboniferous Series of Northumberland." By W. Percy Sladen, Esq., F.G.S., F.L.S.

9. "On the Ancient River-deposit of the Amazon." By C. Barrington Brown, Esq., A.R.S.M., F.G.S.

The author described a series of alluvial deposits, varying in thickness from 10 to 160 feet, which have been cut through by the river, and form a series of cliffs, giving rise to striking and characteristic scenery. The succession of beds exposed in these cliffs was illustrated by a number of sections; and it was shown that the strata in question must have been deposited by river-action. It was then pointed out that the river is performing two classes of work—namely, cutting away the older sheets of alluvial matter, and depositing the materials derived from them at a much lower level. The interesting phenomena of the cutting of curves by the river, and the abandonment by the river of parts of these curves, giving rise to the formation of lakes, was fully explained; and in conclusion the author showed by a map what vast areas in South America have thus been covered by these alluvial deposits.

10. "The Glacial Deposits of Cromer." By Clement Reid, Esq., F.G.S.

The author described the beds shown in the cliffs between Weybourn and Happisburgh. The classification adopted was:—

Sands and Gravels (Middle Glacial?).

Bedded sand and marl.	} Contorted Drift.
Sedimentary Boulder-clay.	

Fine Sands.

2nd Till (unstratified Boulder-clay).

Intermediate Beds (laminated marl &c.).

1st Till (unstratified Boulder-clay).

Arctic Freshwater Bed.

The beds below the 1st Till were not described; the 1st and 2nd Tills were considered to have been formed by land-ice coming from the W. or W.N.W.; while the Intermediate Beds appear to be stratified glacier-mud deposited on the retreat of the ice. The Sands are probably marine; and so also appear to be the Contorted Drift and Middle Glacial. Glacial beds later than the Chalky Boulder-clay appear to be represented near Cromer by valley-deposits which were not treated of in this paper.

In dealing with the question of the age and mode of formation of the contortions, the author pointed out that all, with the doubtful exception of a few of the smaller ones, affected not only the Contorted Drift but also the Middle Glacial, while, at the same time, contortions affecting the overlying beds were often much more complicated at the base, and rested on an even, undisturbed surface of the Preglacial beds. This he accounted for by considering the contortions to be formed by an advancing ice-sheet, which pushed before it a mound of the older glacial beds, while the beds below the level of the base of the ice were undisturbed; thus the junction of the contorted and uncontorted beds is a horizontal fault-line. The ice-sheet he referred to the period of the Chalky Boulder-clay.

The author drew attention to the fact that the hollows in which the Middle Glacial gravels rest are in every instance due to contortion, and not to erosion. The large masses of chalk in Boulder-clay were considered to have been torn off by the same force that produced the contortions.

11. "On a Disturbance of the Chalk at Trowse, near Norwich." By Horace B. Woodward, Esq., F.G.S.

Attention was drawn to a section at Trowse, where the Chalk with bands of flint was uplifted at an angle of about 37°. Abutting against the Chalk was a mass of the same rock rearranged, containing broken flints, and pebbles of flint, quartz, and quartzite. This rearranged Chalk was traced underneath the uplifted beds; and the author gave reasons for believing that the disturbance was produced by the agent (land ice) which formed the Chalky Boulder-clay. Allusion was made to many cases of glaciated Chalk in Norfolk, and to the cervine and other organic remains occasionally met with in it. Comparing the Trowse section with that at Litcham, described by Mr. S. V. Wood, Jun., the author came to the conclusion that this was a similar and striking instance of the incipient formation of a huge Chalk boulder, serving to throw light on the origin of the large transported masses seen in the Cromer cliffs.

12. "The Submerged Forest of Barnstaple Bay." By Townshend M. Hall, Esq., F.G.S.

13. "On a Section of Boulder-clay and Gravels at Ballygalley Head. By T. Mellard Reade, Esq., C.E., F.G.S."

The section described is in a gravel-pit about 30 feet deep, situated between Larne and Cushendall, in the north of Ireland. It shows a considerable thickness of false-bedded gravel and sand, containing shells; covered by Boulder-clay. The shells collected from the sand and gravel of this section do not, however, agree with those found in the so-called "Interglacial beds" of Lancashire; and the author regards the section as confirming his views that the tripartite division of the drift-deposits is not, as maintained by Hull and other geologists, equally applicable to the Irish and Lancashire deposits.

14. "On the Augitic Rocks of the Canary Islands." By Prof. Salvador Calderon. Communicated by the President.

As the result of a long investigation of the eruptive rocks of the Canaries, and especially of Las Palmas, the author has come to the conclusion that there are two groups of such rocks in those islands—an older one, characterized by the presence of hornblende, and a newer, containing augite. In the latter he finds the essential minerals to be plagioclase, augite, magnetite, olivine, sanidine, and nepheline; and he distinguishes among them the following kinds of rocks, all of which have their characteristic minerals imbedded in a paste of augite and plagioclase:—1. *Augite-andesite*, with a small quantity of sanidine; 2. *Tephrite*, with no sanidine, but abundance of nepheline; 3. *Basanite*, with some peridote; 4. *Nepheline-basalt* with abundance of peridote; 5. *Dolerite*, crystalline, characterized by the disappearance of nepheline, the abundance of peridote and porphyritically imbedded plagioclase, augite, and olivine; 6. *Felspathic basalt* (like 5, but semicrystalline); and 7. Essentially olivine modern lavas.

15. "On the Cambrian (Sedgw.) and Silurian Beds of the Dee Valley, as compared with those of the Lake-district." By J. E. Marr, Esq., B.A., F.G.S.

The principal differences in these districts appear to be due, (1) to the non-correspondence of the epoch of volcanic activity, (2) to the upheaval and consequent denudation of a part of North Wales after the end of the Cambrian period. The author correlates the deposits in the two areas, the Dee valley having deposits parallel to the various members in the Lake-district, from the Skiddaw Slates to the Upper Coldwell beds inclusive. The Lower Bala series contains ash-beds which appear to be andesite. In the Middle Bala of Wales are some fossils which, in the Lake-district, occur in the Ashgill Shales above the Coniston Limestone. He finds the equivalents of the Graptolitic Mudstone of the Lake-district, near Carrig-y-druidion, correspondent in stratigraphical position, lithological character, and fossils. One Graptolite, *G. colonus*, however, occurs at a higher horizon in the Lake-district. In the lowest division of the Denbigh Grits *Acroculia haliotis* is found, which, in the Lake-district, occurs in the Upper Coldwell beds; and in the upper-

most series there are other fossils which occur in the Bannisdale Slates and Kirby-Moor Flags, thus giving further indications of a northward migration.

16. "On some Superficial Deposits in the Neighbourhood of Evesham." By the Rev. A. H. Winnington Ingram, M.A., F.G.S.

17. "Descriptions of Palæozoic Corals from Northern Queensland, with Observations on the Genus *Stenopora*." By Prof. H. A. Nicholson, M.D., D.Sc., F.G.S., and R. Etheridge, Jun., F.G.S.

LXIX. Intelligence and Miscellaneous Articles.

ON A PILE WITH CHLORIDE OF LIME. BY ALF. NIAUDET.

THE pile which I submit to the Academy has for its positive electrode a plate of zinc, and for its negative electrode a block of carbon surrounded by carbon fragments. The zinc is immersed in a solution of chloride of sodium, the carbon in chloride of lime in a porous vessel of porcelaine dégourdie or of parchment paper. Chloride of lime is, as is known, a mixture of lime and hypochlorous acid; this substance appears very suitable for depolarizing the carbon electrode, since both of its elements can combine with hydrogen to form water and hydrochloric acid. This acid attacks the zinc and forms chloride of zinc, or the lime and forms chloride of calcium; these two salts are highly soluble and are very good conductors.

It will be seen that all the combinations produced are soluble; moreover, if salts be formed, or substances of complex composition, as happens in nearly all piles, they are soluble, as experiments dating from three years since have shown me.

Zinc in presence of chloride of lime is not attacked appreciably; consequently the piles in which they are associated can be left for an indefinite time at rest without wear; the action commences only when the circuit is closed. This property is, as is known, of capital importance for a great number of applications.

We must also justify the employment of sea-salt: its being less expensive is not the only advantage it presents; it is one of the most conductive liquids known. Besides, we have tried other chlorides—particularly sal-ammoniac and chloride of lime,—and other liquids, hydrochloric and sulphuric acids for instance: they have all given less satisfactory results and less powerful electromotive forces than sea-salt. The reasons of these inferiorities are doubtless various; as regards sulphuric acid, they appear to depend on the formation of insoluble sulphate of lime, which interferes with the ulterior reactions.

The electromotive force at the commencement was found to be above 1.6 volt; it was more than 1.5 after the pile had been left for several months.

The depolarization produced by the chloride of lime is not complete as in the pile with sulphate of copper: if the current be made to pass continuously with a weak external resistance, the electromotive force diminishes, as happens with almost all piles. But that force resumes its first value in a short time, as seen in the following experiment:—The current-circuit of one element was closed during 40 minutes, with an external resistance of 1 ohm; the elec-

tromotive force, originally = 139, fell to 113; but a rest of 40 minutes raised it to 129, and at the end of two hours it was at 138.

We have taken great care to reduce as much as possible the internal resistance of the element: the zinc surrounds the porous vessel at a very little distance; and we avoid touching it by interposing two rings of pack-thread.

The odour of the chloride of lime is not perceptible, because the vessel is closed with a cork covered with pitch, which prevents the liquid from being spilled in conveyance, and the salt from evaporating. One perforation only is made in the cork, for pouring the water into the pile at the moment of setting it in operation.—*Comptes Rendus*, Oct. 27, 1879, t. lxxxix. pp. 703–705.

ON THE ABNORMAL SPECTRUM OF LIGHT.

BY M. DE KLERCKER, OF STOCKHOLM.

The anomaly produced in the spectral position of various rays of light when dispersion is occasioned by solutions of certain colouring matters has for several years past deservedly attracted attention.

The rays, of different sorts, whose index of refraction increases in ordinary cases in proportion as the length of the luminous waves diminishes from one end of the spectrum to the other, are here thrown out of their normal positions; moreover this change of position may be carried to such a degree that the usually most refrangible rays, the blue and the violet, appear the least refrangible of all, as is best proved when the light is dispersed by means of a solution of fuchsine (aniline red).

There is, however, nothing in this phenomenon which would disclose in a decisive manner the presence of an irregularity in the dispersion which would be called forth by the molecules of fuchsine acting alone. But if we must seek elsewhere the origin of the anomaly, we shall be led to admit that the refraction of light by the fuchsine solution takes place for certain rays on quite another scale than for the rest; and as this property of the solution is a consequence of the presence of the molecules of the colouring material, we may thence conclude that these must possess the hitherto unknown property of retarding only certain luminous rays while they permit the others to pass freely.

This explanation appears to me to have been completely verified by the spectral analyses to which I have devoted myself in the course of the past summer, the first results of which I communicate today.

Two hollow prisms of plate glass, both having the same refracting angle of 25° , were placed on the platform of a spectroscope, one behind the other, and the refracting angles directed oppositely. The two prisms having been filled with alcohol, no deviation was impressed upon the image of the slit of the spectroscope, on account of the two refractions being equal and of contrary signs.

I then proceeded by degrees, adding successively to the alcohol of one of the prisms small quantities of crystallized fuchsine, leaving the alcohol of the second prism pure. The following were the observed effects:—

The primitive image became divided into two parts, which sepa-

rated more and more, each presenting an altogether special optical property. One of these parts went pretty quickly to the right of the cross-thread, increasing little by little in breadth, and finally became a distinct spectrum containing in the *regular* order the less-refrangible rays red, orange, and yellow, with Fraunhofer's lines A, B, C, and D distinctly visible; while the other part, which assumed a violet-blue colour at the moment of separation, remained still in its original place during the progress of the concentration, and consequently retained always the same breadth as the primitive image.

From the above-mentioned observations, made with all possible care, repeated several times, and always with the same results, the following conclusions may be drawn:—

“The abnormal spectrum of light is composed of two perfectly separate parts, doubtless due to the different amounts of retardation produced by the different kinds of molecules contained in the solution.”

We have seen that one of these parts, that which comprises the less-refrangible rays, *removes regularly* from the normal to the surface of separation between the two prisms in proportion as the concentration of the fuchsine solution is augmented in the anterior prism. Now this proves evidently that the molecules of fuchsine retard the less-refrangible rays, while they let the others pass freely. In short, as the rays of this portion are all disposed in perfect accordance with the law of dispersion, nothing abnormal is produced here.

We have also seen that the other part of the abnormal spectrum (that containing the more-refrangible rays) *neither moves further from nor comes nearer to* the normal during the concentration of the fuchsine solution. That proves that this portion of the rays is retarded exclusively by the molecules of the dissolvent, although the retardation is not appreciable when two opposed prisms are employed. Therefore neither in this part of the phenomenon is there any real anomaly.

The dark space between the two portions of the abnormal spectrum is, then, nothing else but the interval separating two wholly distinct spectra.—*Comptes Rendus*, Nov. 3, 1879.

EXPLOSION OF A DIAMOND.

Prof. Leidy exhibited a black agate sleeve-button, having mounted upon it, centrally in a raised gold band, a rose diamond about 7 millims. broad. It had been submitted to him by Mr. Ernst Kretzmar, jeweller, who informed him that the person who wore it was recently leaning with his head upon his hand, on a window-ledge in the sun, when the diamond exploded audibly, and with sufficient force to drive a fragment into his hand and another into his forehead. On examining the diamond, the fractured surface, following a cleavage-plane, exhibits apparently the remains of a thin cavity, such as is sometimes seen in quartz crystals. The fracture also exposes a conspicuous particle of coal. Prof. Leidy thought that the explosion had been due to the sudden expansion of a volatile liquid contained in the cavity, as frequently occurs in cavities in many minerals.—*Proceedings of the Academy of Natural Sciences of Philadelphia*, July 22, 1879.

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END OF THE EIGHTH VOLUME.

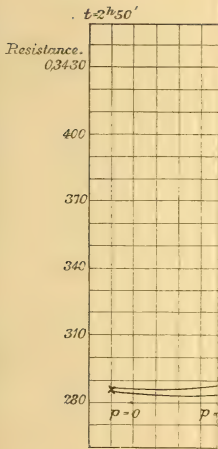


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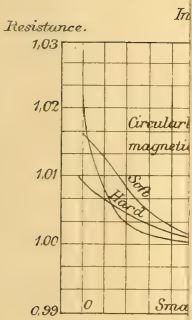
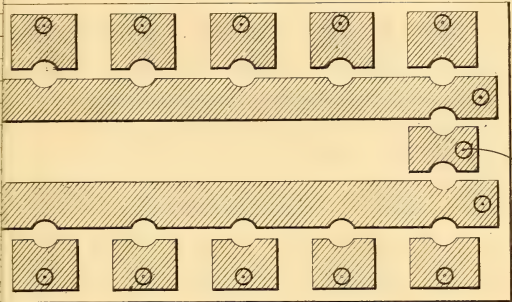


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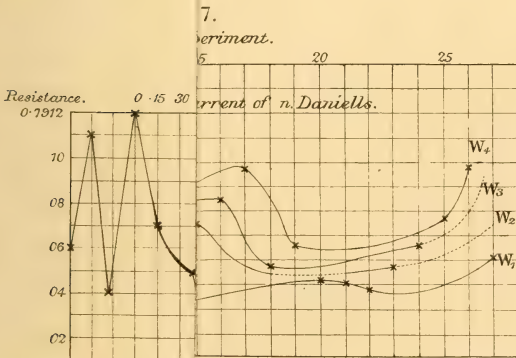
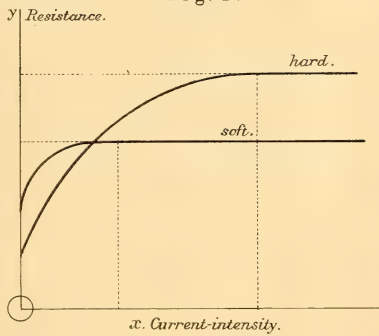


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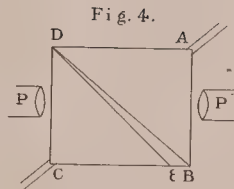
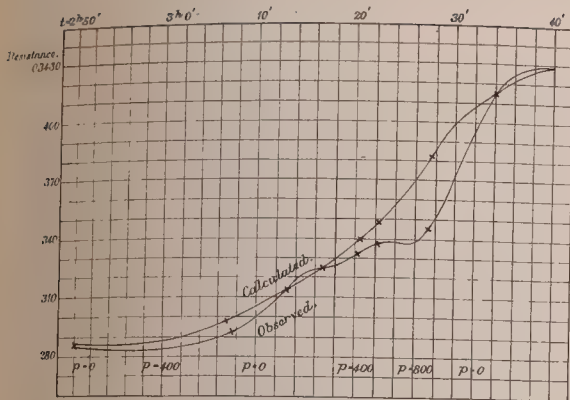


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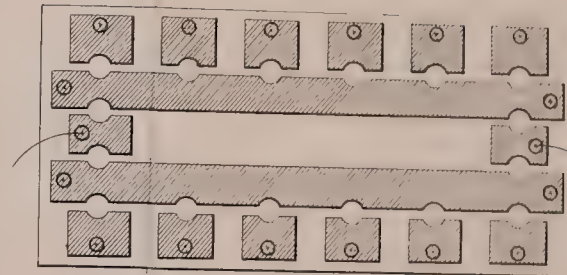


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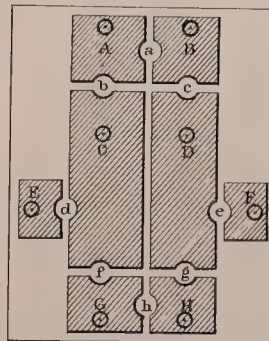


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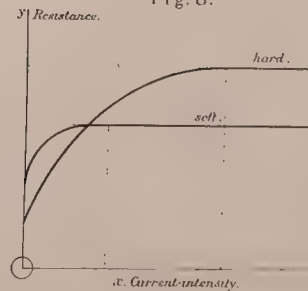


Fig. 2.

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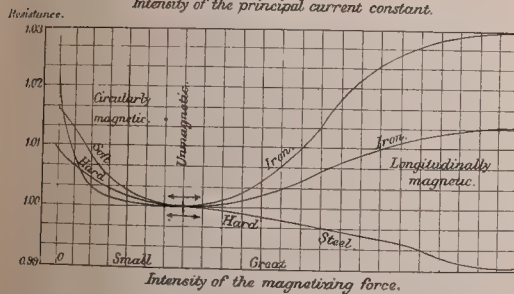


Fig. 3.

Seconds.

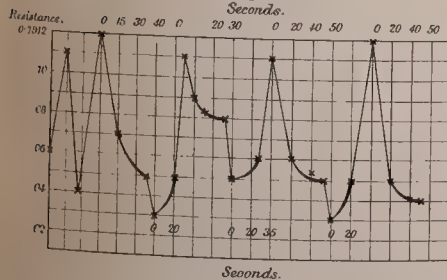


Fig. 7.

Nº of the Experiment.

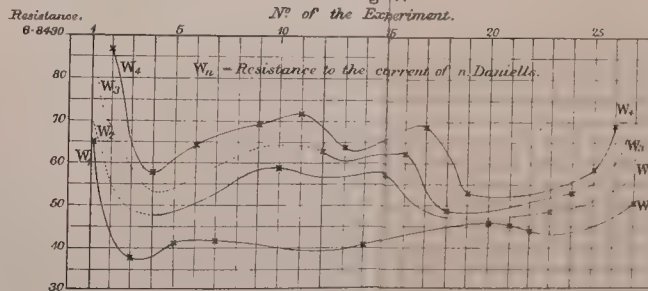


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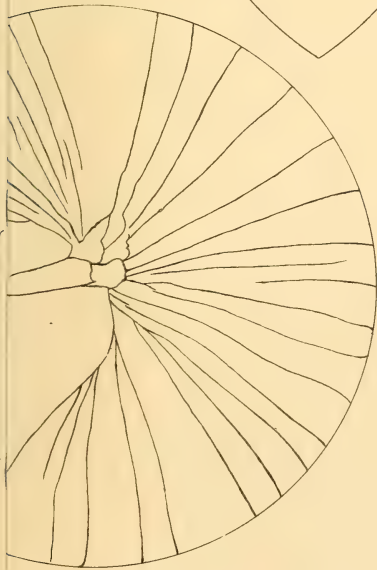


Fig. 2.

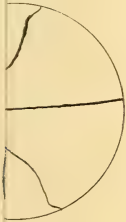


Fig. 3.



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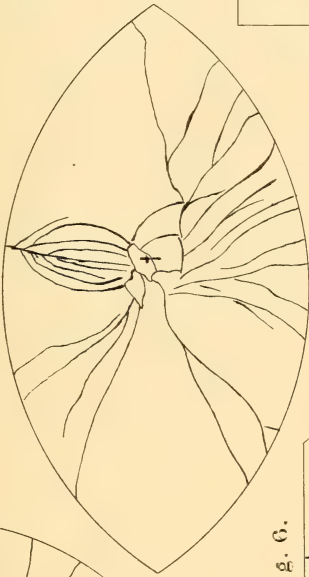


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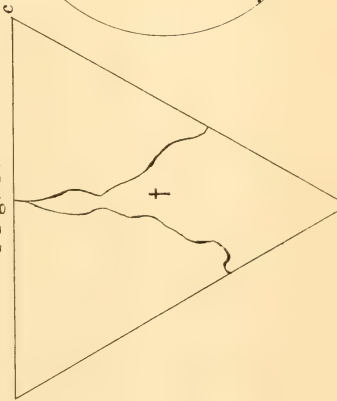


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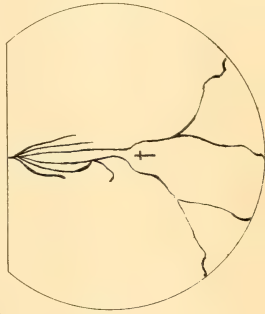


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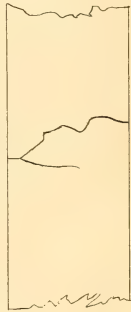


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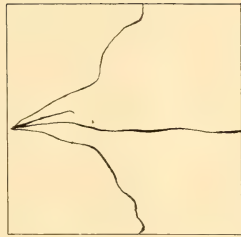


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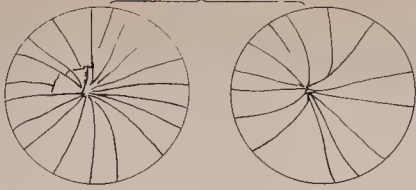


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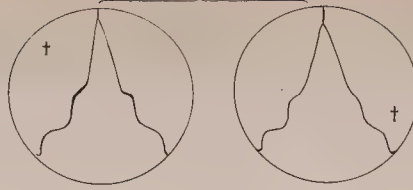


Fig. 3^a



Fig. 3.

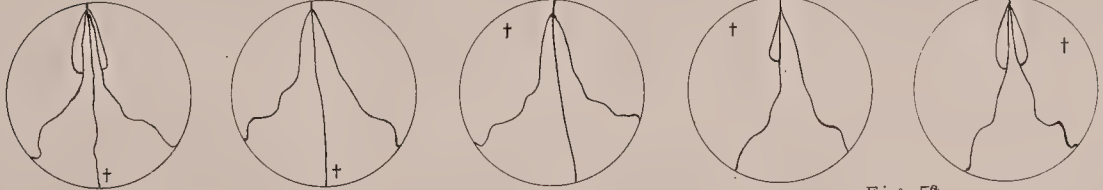


Fig. 4^b

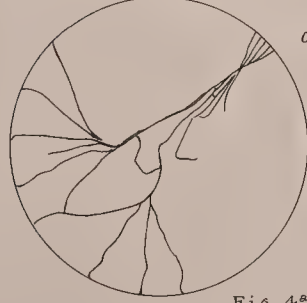


Fig. 4^a



Fig. 4^c

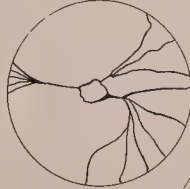


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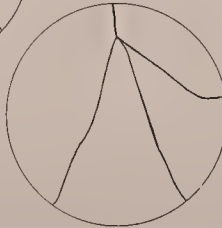


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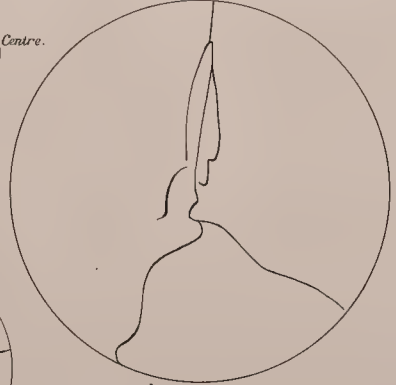


Fig. 5^b



Fig. 6.

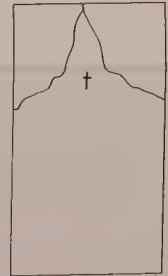


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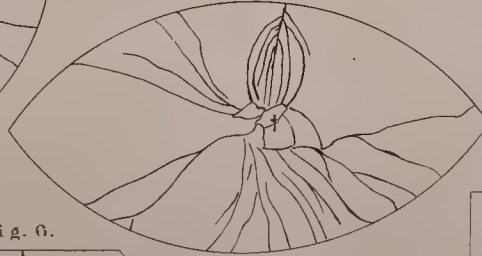


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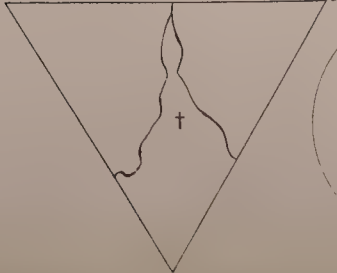


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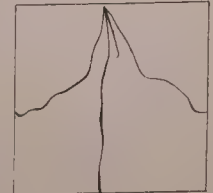


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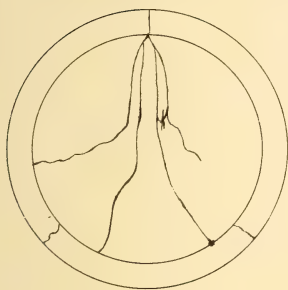


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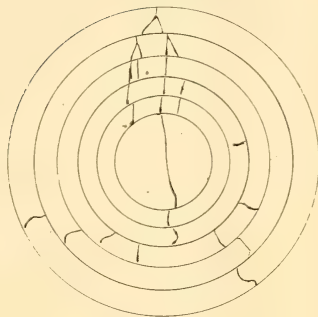
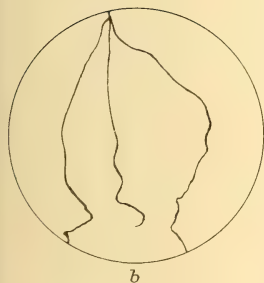
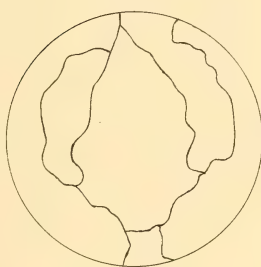


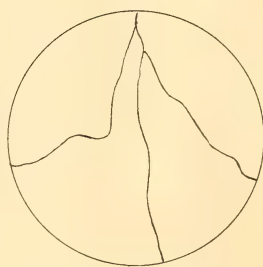
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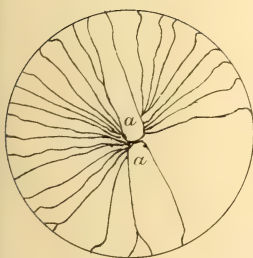


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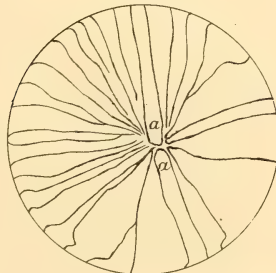
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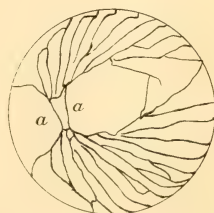
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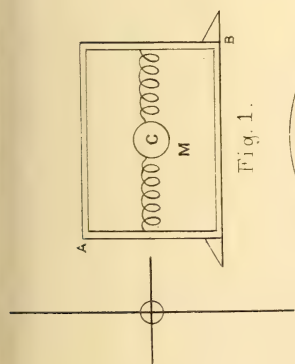


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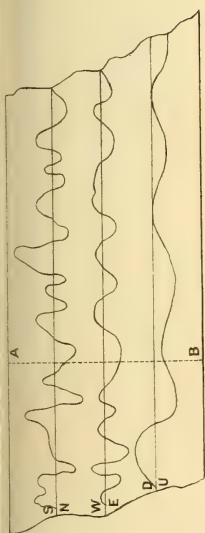


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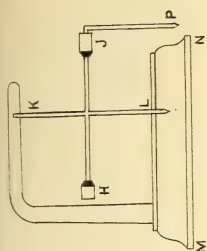


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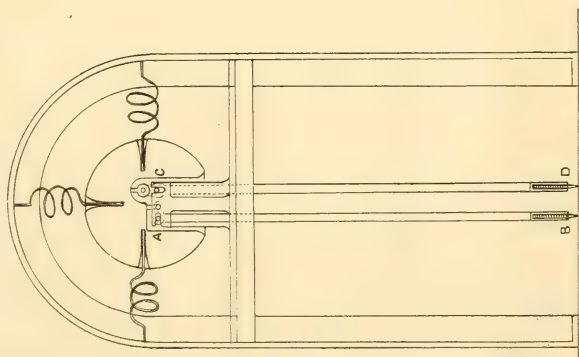


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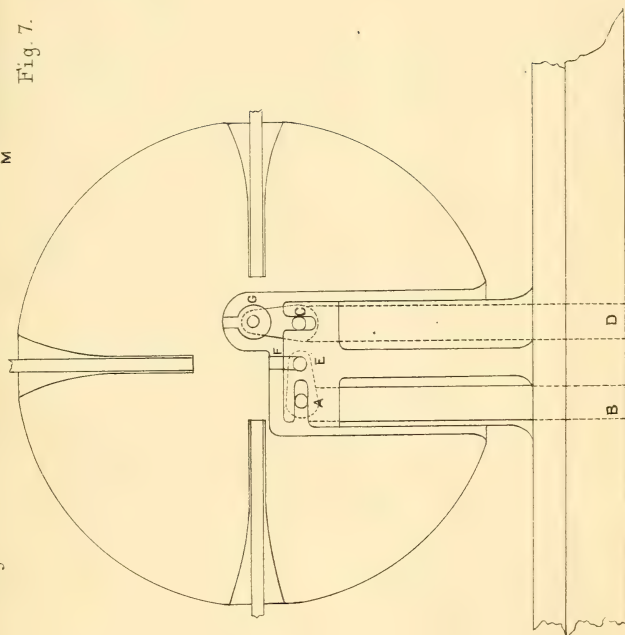
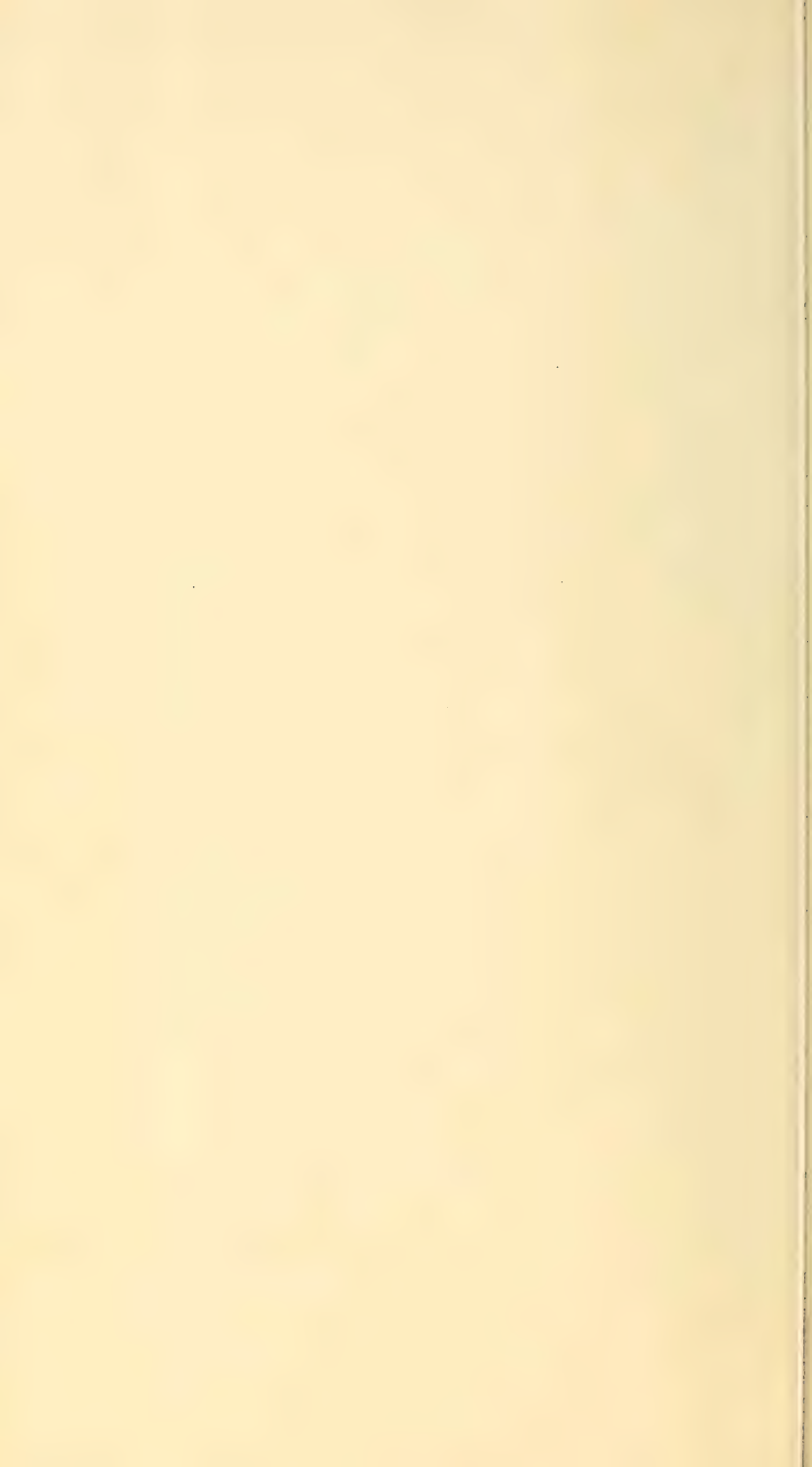
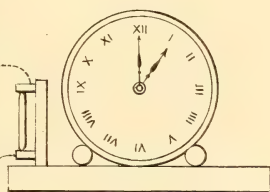
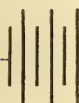
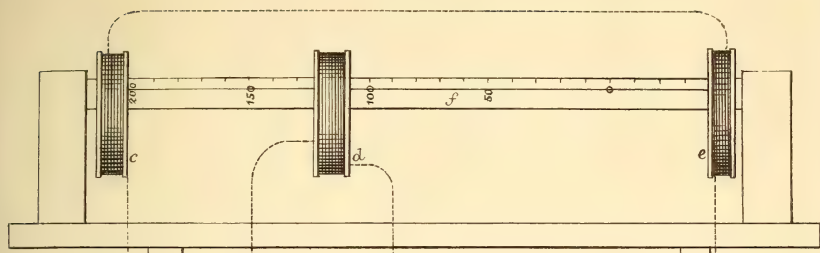


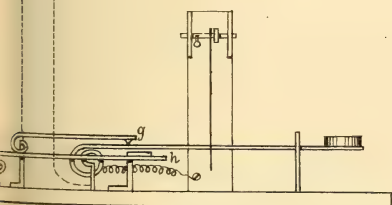
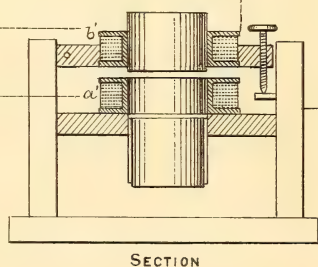
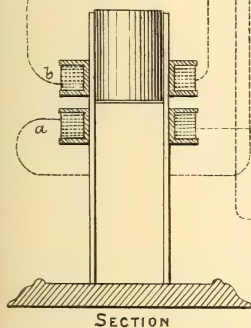
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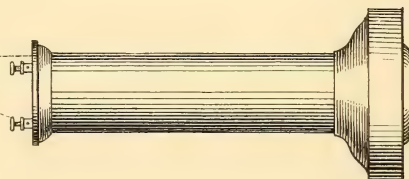
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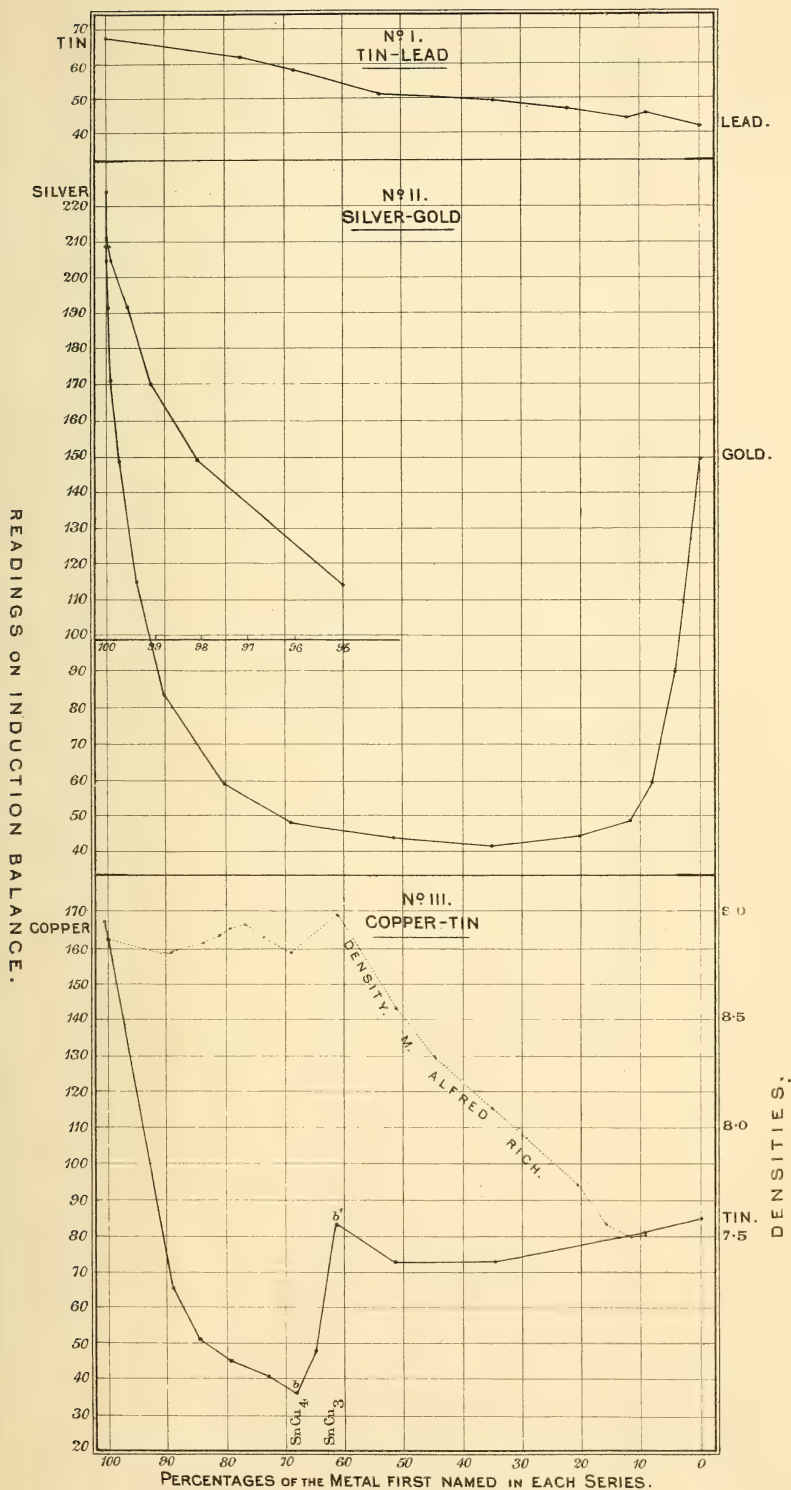
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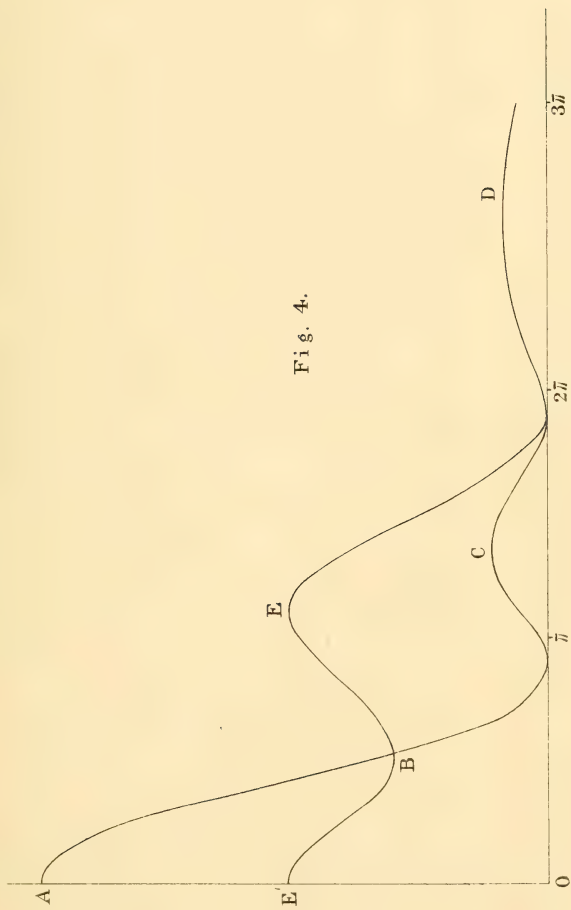


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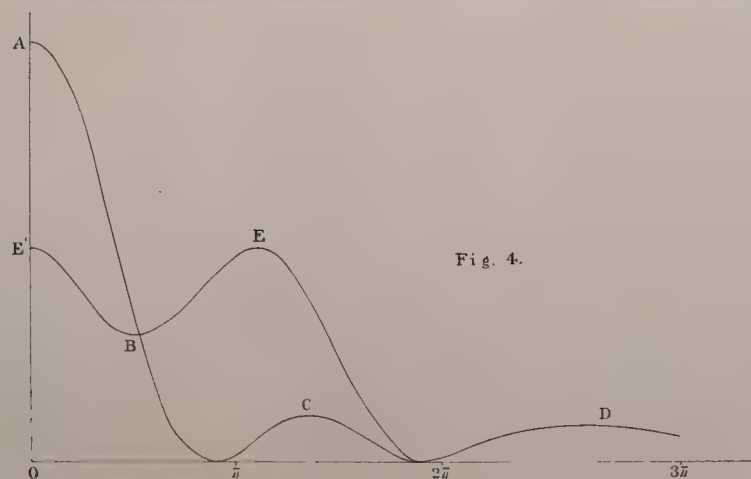
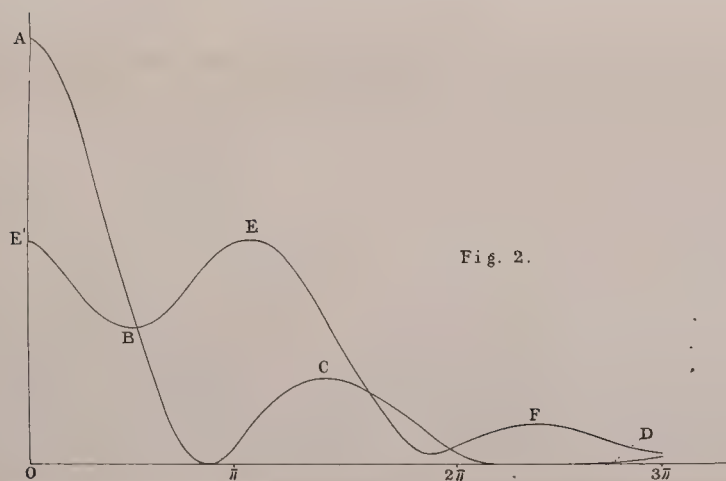
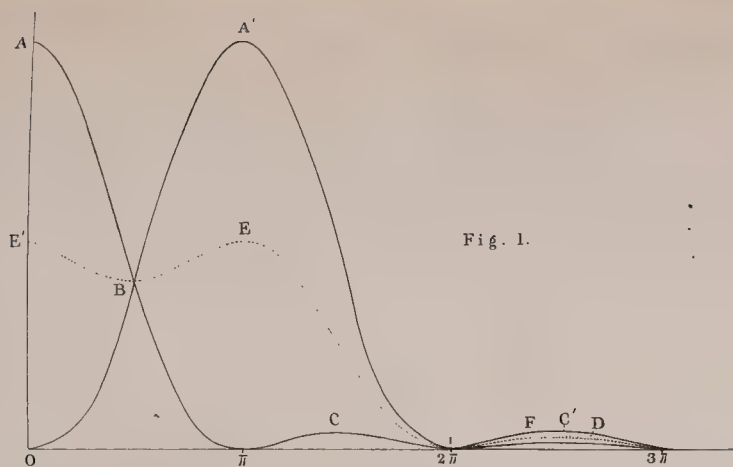


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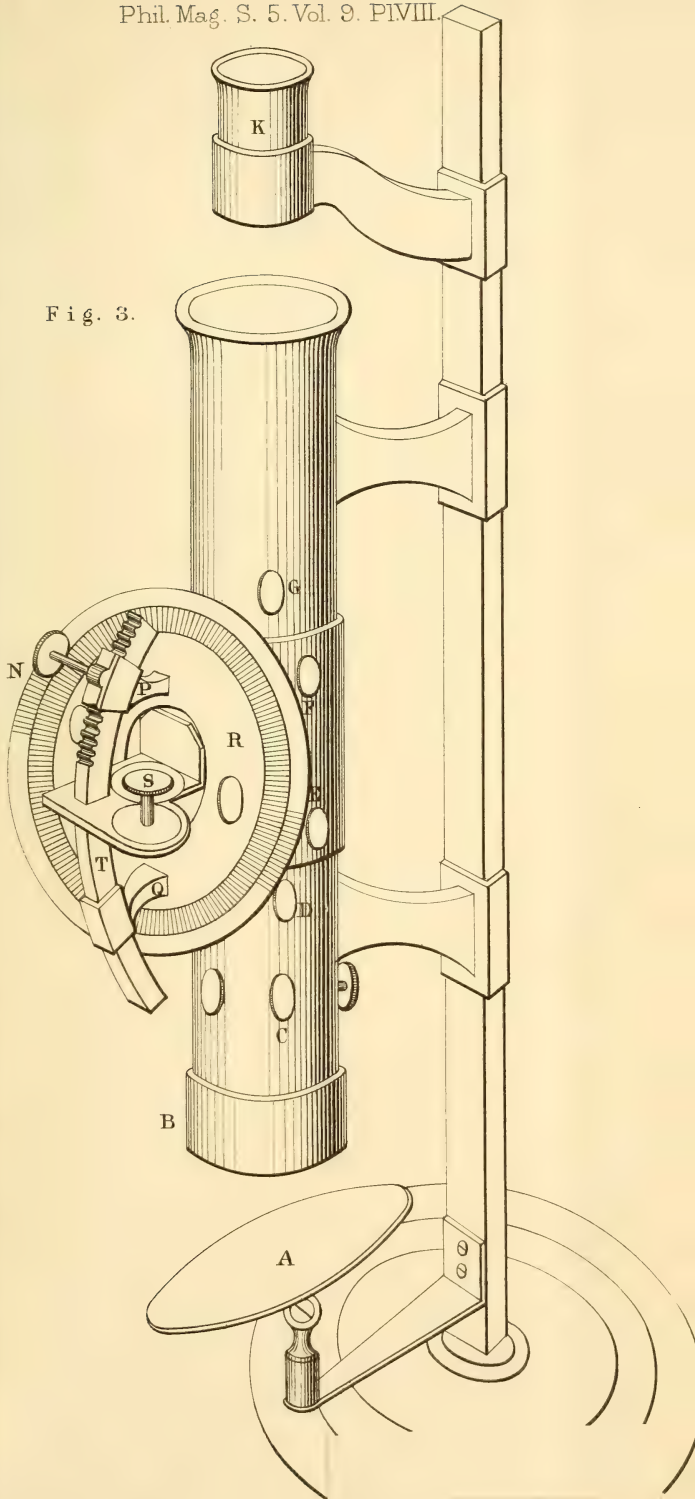


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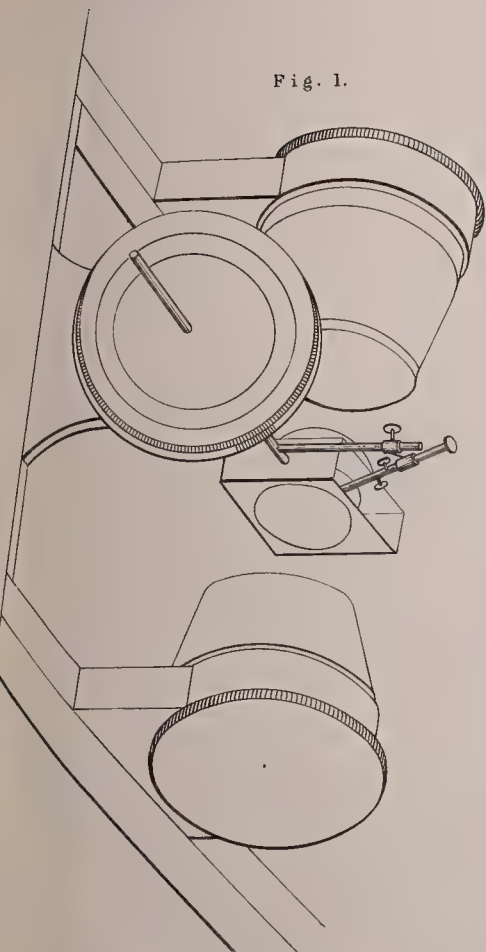


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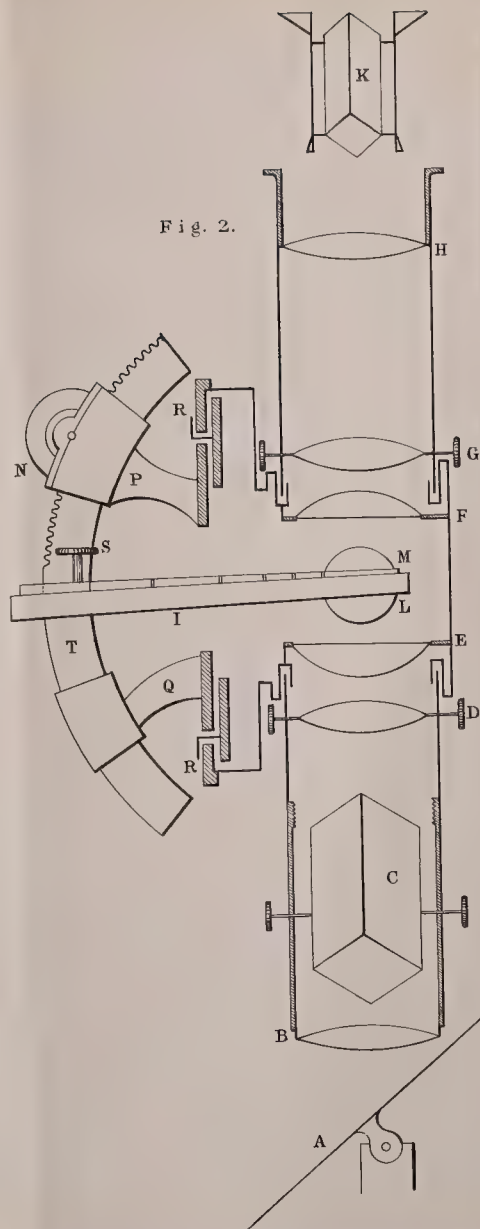
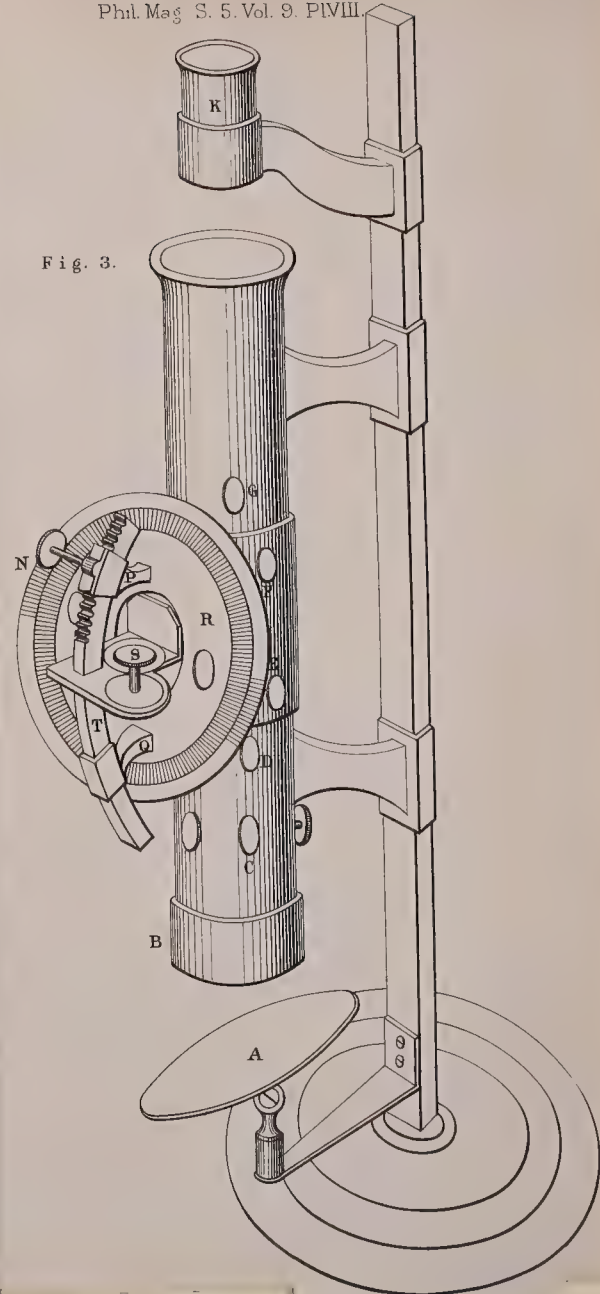


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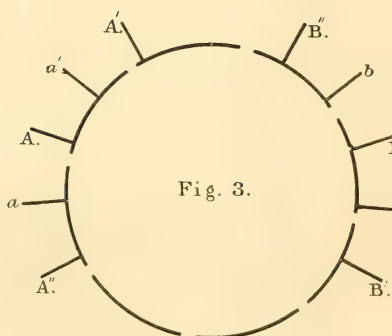
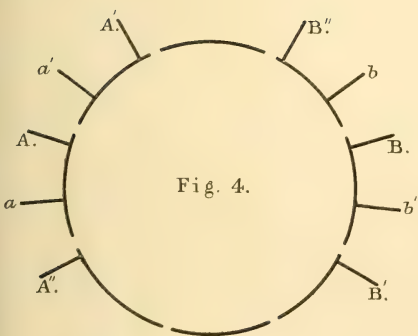
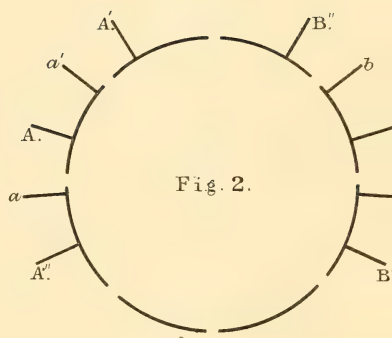
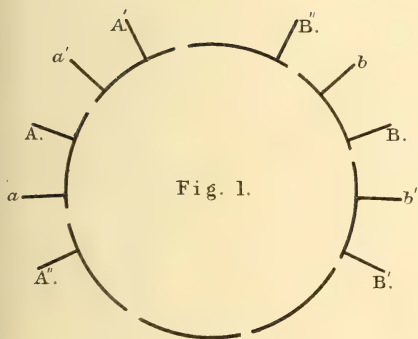


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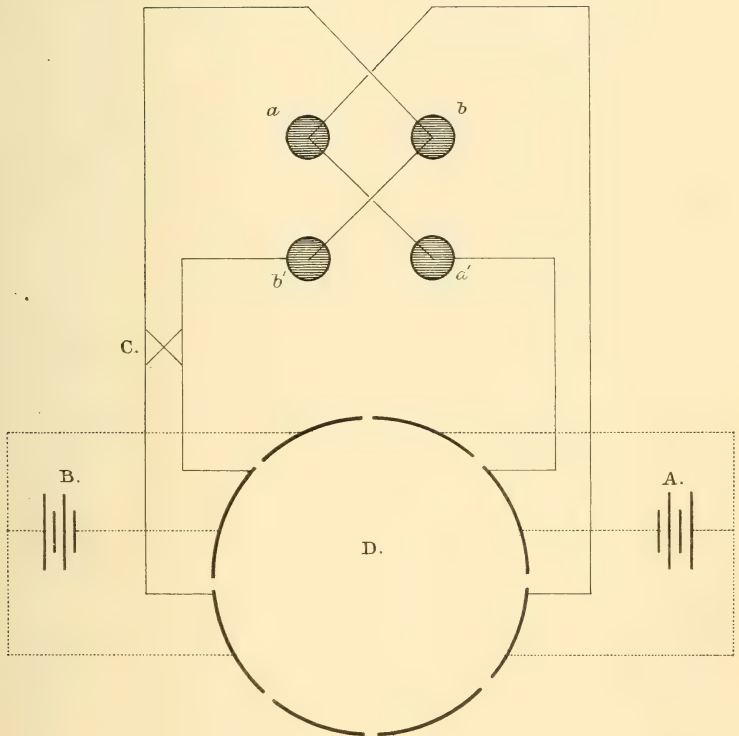


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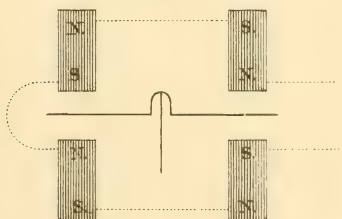
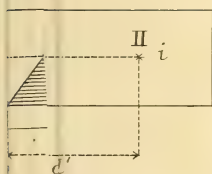
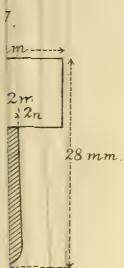
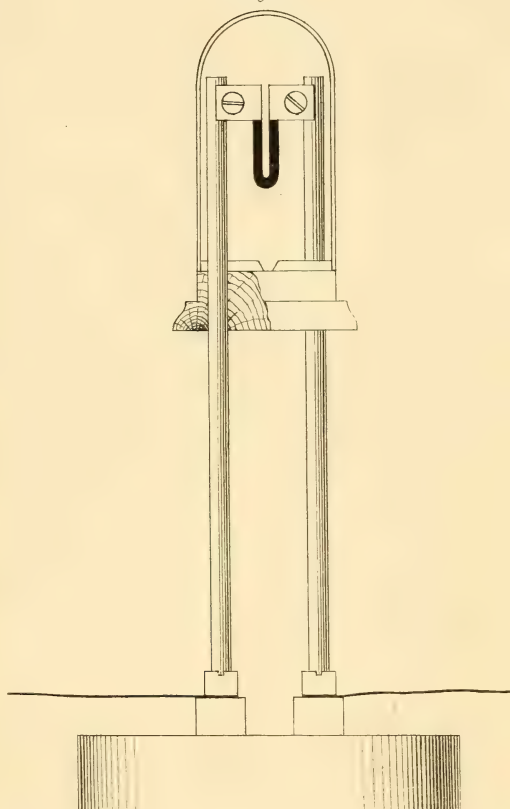
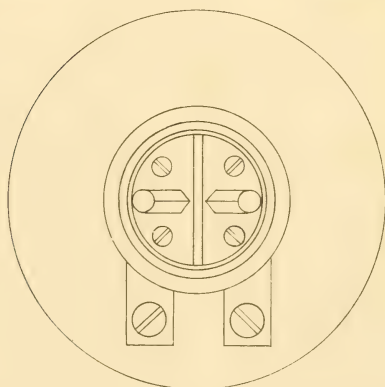
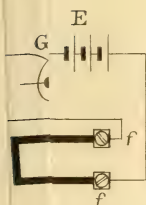
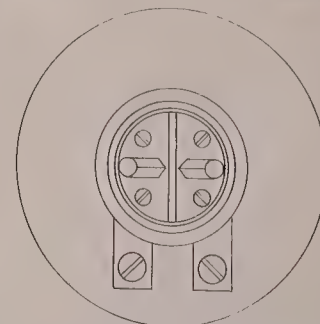
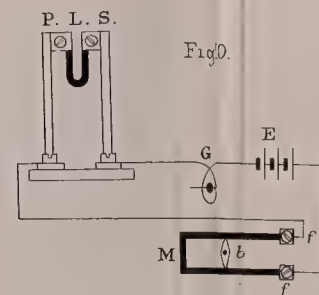
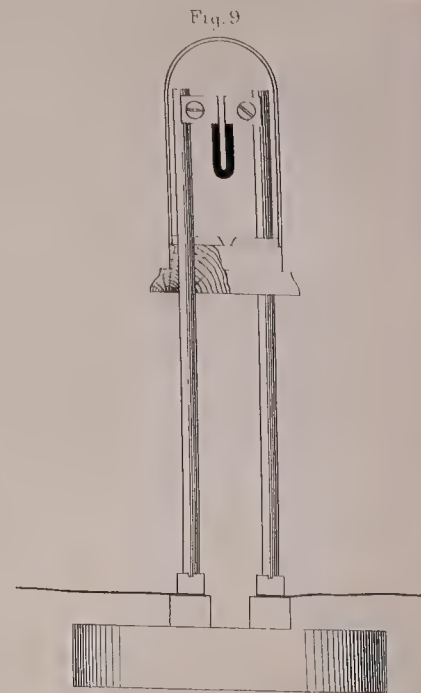
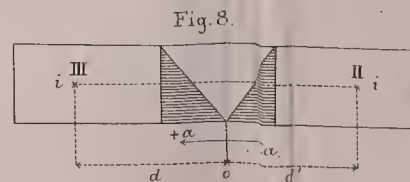
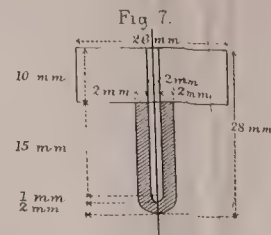
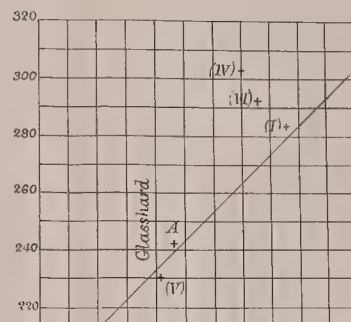
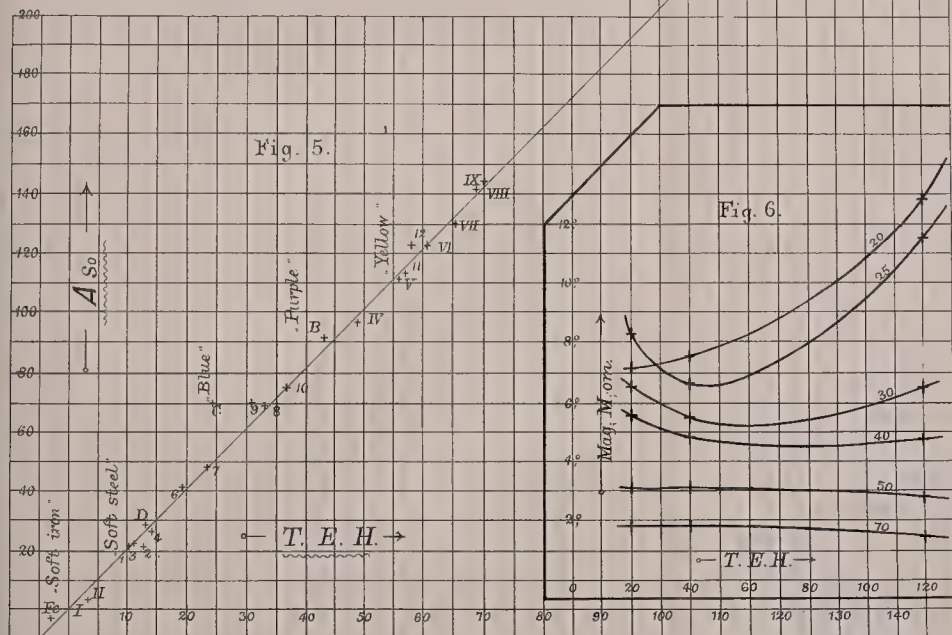
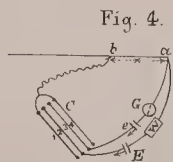
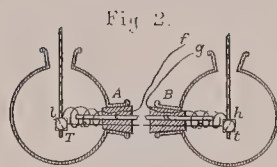
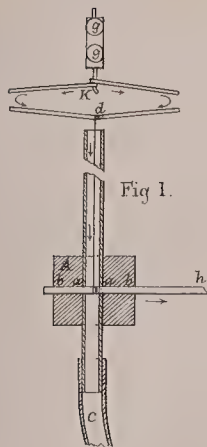


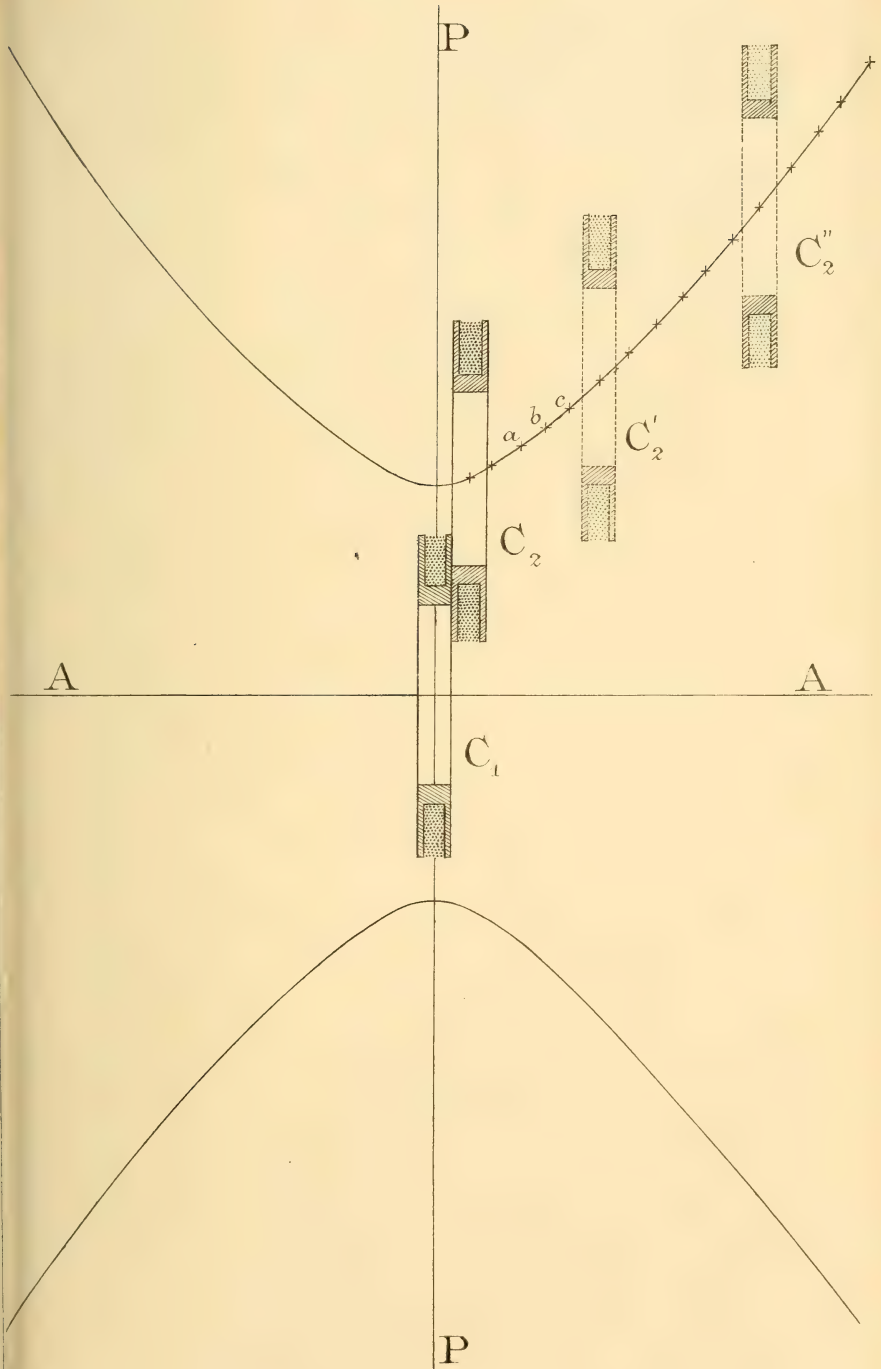
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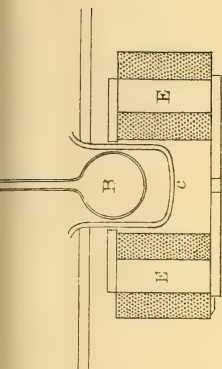


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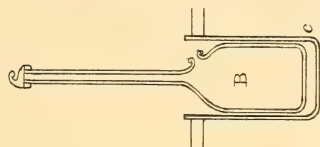


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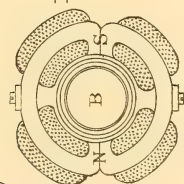


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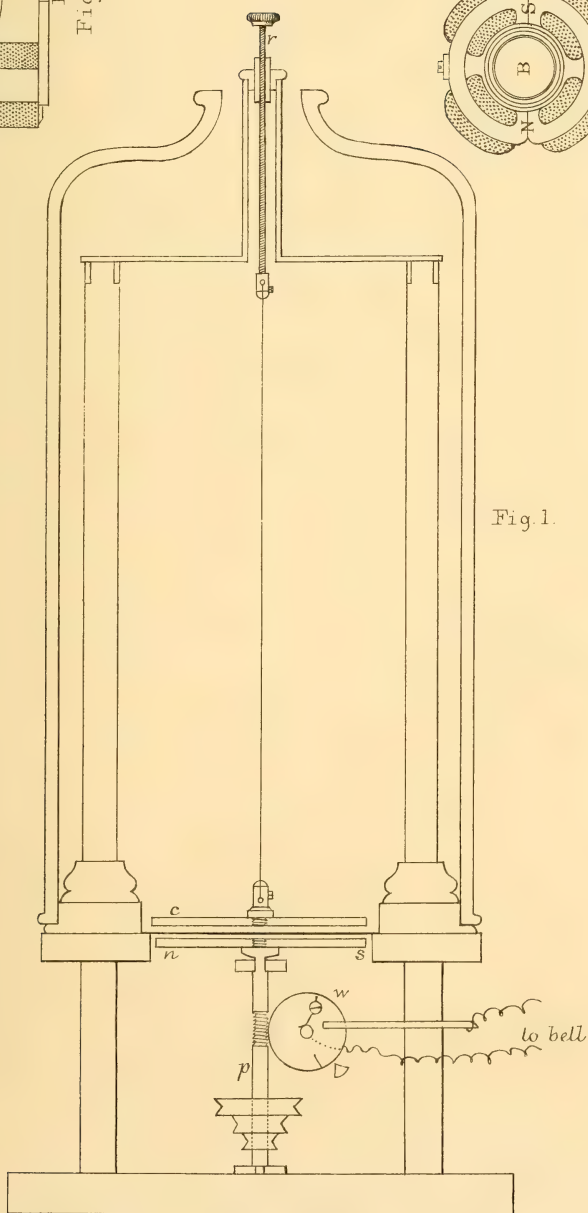


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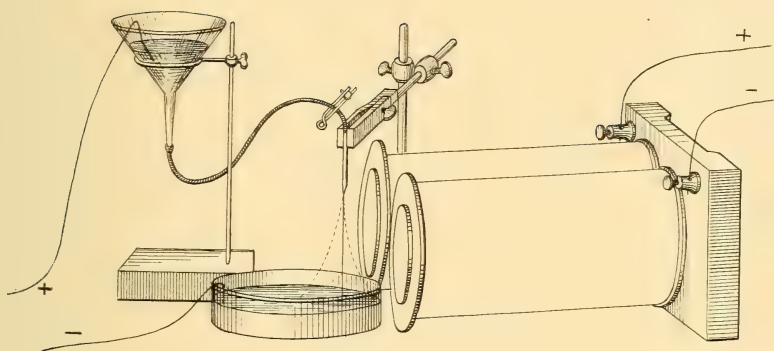


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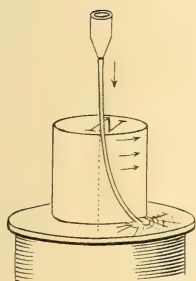


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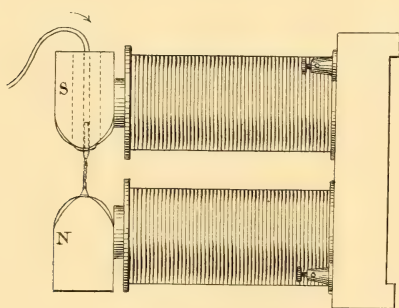


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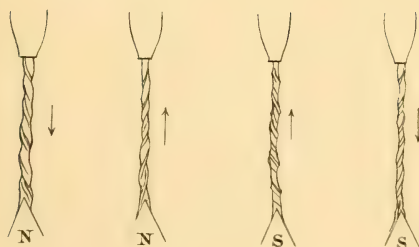


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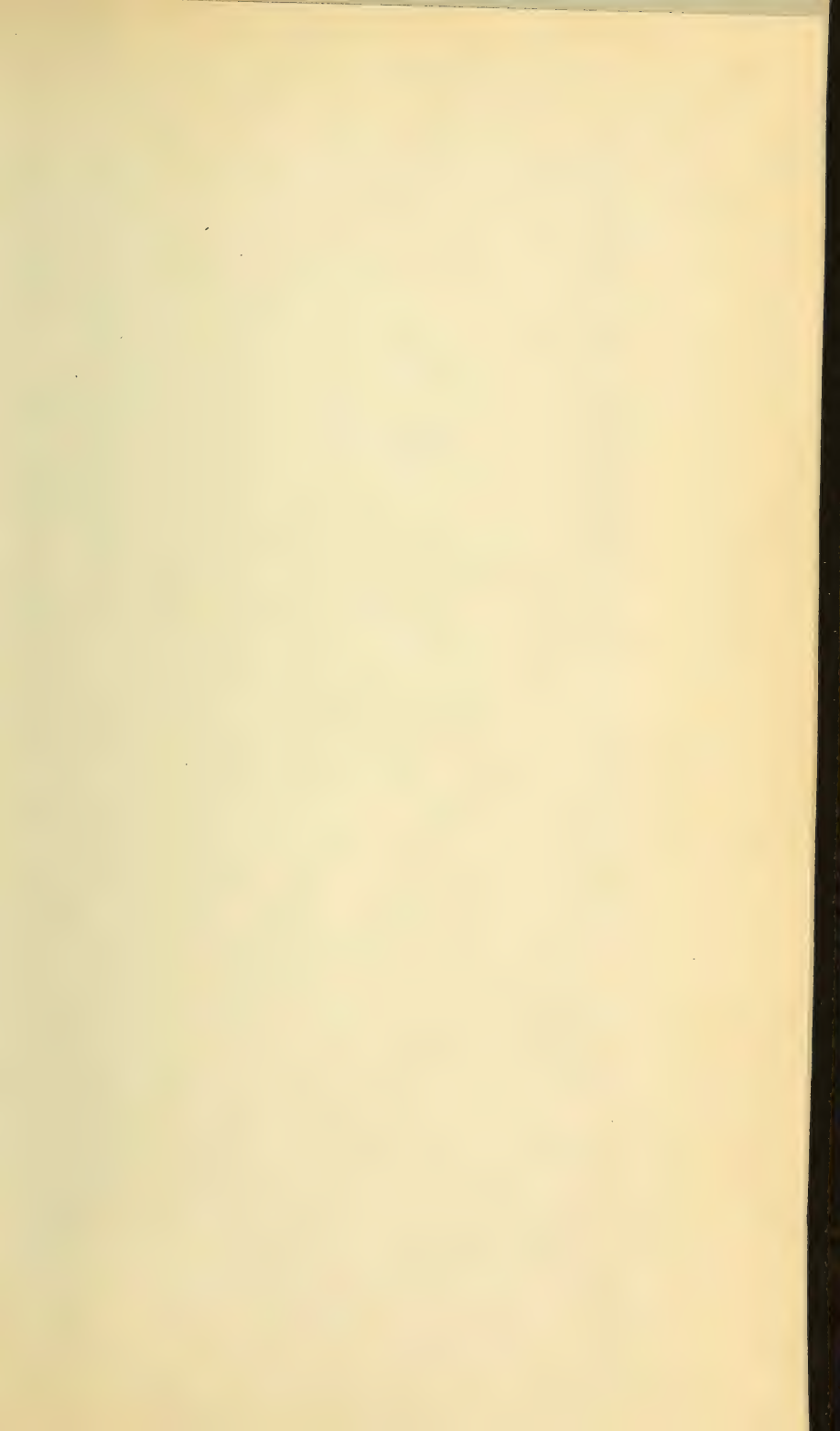
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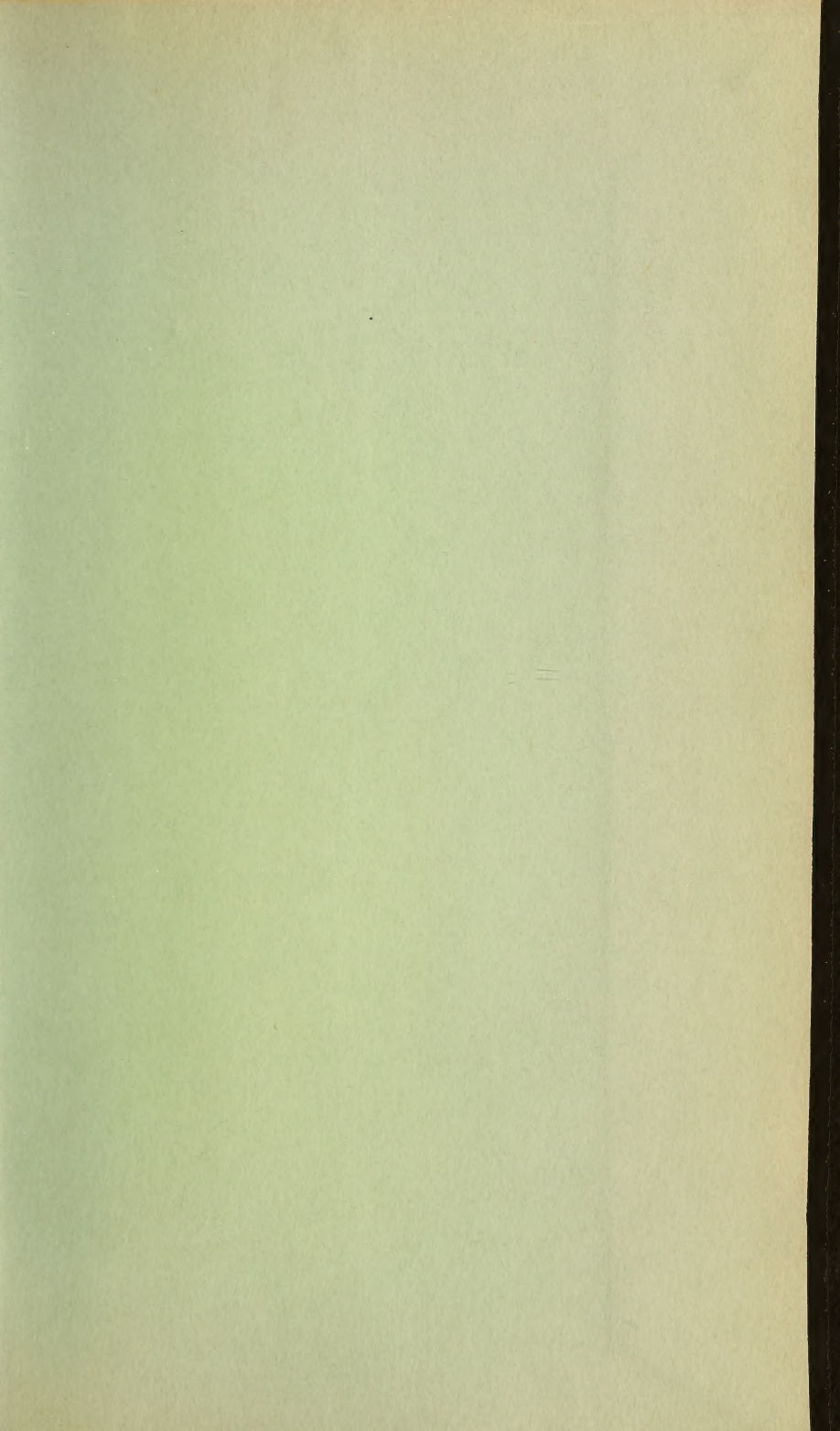
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